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TECHNICAL ASPECTS OF THE LOSS OF THE U.S.S. *SHENANDOAH*.

As is generally known, the U.S.S. *Shenandoah* left the mooring mast at Lakehurst, New Jersey, on September 2, 1925, and proceeded on a flight to the mid-west. On the morning of September 3, 1925, the ship became involved in a storm which resulted in her total destruction in Noble County, Ohio.

A Court of Inquiry was convened by the Secretary of the Navy to make a thorough investigation into all the circumstances connected with the loss of the *Shenandoah*, the causes thereof and responsibility therefor. The Court was directed to include in its findings a full statement of the facts it might deem to be established, and to give its opinion as to whether any offenses had been committed or serious blame incurred, and if so to recommend specifically what further proceedings should be had.

The Court was composed of Rear-Admiral Hilary P. Jones, U. S. Navy; Captain Lewis B. McBride (Construction Corps),

U. S. Navy, and Commander John H. Towers, U. S. Navy. Captain Paul Foley, U. S. Navy, who was Judge Advocate originally, was later relieved by Major Henry Leonard, U. S. Marine Corps, Retired.

The Court assembled on September 21, 1925, and held sessions on thirty-one days; examined some sixty-five witnesses, including all of the survivors of the *Shenandoah* except one who was ill in a hospital, and including experts of wide reputation. About 2100 pages of testimony was included in the Court's record, in addition to numerous technical reports, papers, diagrams, charts and photographs.

Through the courtesy of the Navy Department, the record of the Court has been made available for publication by the American Society of Naval Engineers. In order that the technical information contained in the voluminous record may be made available to the engineering and scientific world, abstracts have been made of such testimony, reports, diagrams, and photographs as will give a coherent account of the various phases of the destruction of the *Shenandoah*.

While it would perhaps be of interest to publish the entire record of the Court, there is considerable testimony not of a technical nature which would be out of place in the JOURNAL. Where testimony of such a nature given before the Court has not been abstracted, it is referred to in sufficient detail to bring out its value or lack of value.

The abstract as herein published contains the following:

Narrative report of the senior survivor of the wreck, as required by the Navy Regulations.

Testimony of survivors who landed in the two parts of the ship on events immediately preceding her destruction.

Testimony of the aerological officer on the weather at the time of the wreck.

Views of the wreckage.

Testimony of experts on gas cell pressures, etc.

Testimony in regard to the duralumin structure of the ship.

Technical argument of Professor William Hovgaard — Adviser to the Judge Advocate.

Brief submitted by the survivors.

Remarks of the representative of the Chief of Bureau of Aeronautics.

Findings of the Court.

In investigating the loss of the ship, the technical inquiry proceeded on the following lines, after receiving an account of actual occurrences on the voyage until the several parts reached the ground:

Were the weather conditions forecast properly and was the *Shenandoah* handled efficiently in view of the expected weather?

Did the change in the valving arrangements of the gas cells of the ship have any bearing on her loss?

Was the designed strength of the duralumin structure of the *Shenandoah* sufficient to meet expected aerodynamic stresses?

Had any deterioration developed in the duralumin structure of the ship which would account for her failure to withstand the conditions met?

The *Shenandoah* had a structural hull of streamline form with a length overall of 680.15 feet and a maximum diameter of 78.74 feet. The cross section was approximately a regular polygon of twenty-five sides. The structure consisted of latticed struts or girders of duralumin braced by steel wires. There were thirteen principal longitudinal girders, extending from the bow to the stern, and twelve intermediates at the vertices of the polygon, with diagonal wiring between them. The main transverse frames were spaced ten meters apart and braced by radial and chord wires in their own planes, with an intermediate cross frame, not braced by wiring, between every two main frames. At the bottom of the airship a system of special longitudinal and transverse girders constituted a keel or corridor of triangular cross section with a walkway; at the forward end of the hull means for mooring the ship to a mooring mast were provided and at the after end there were vertical and horizontal fins and rudders. There were six twelve cylinder Liberty engines of 300 horsepower each in the original design, each in a separate car driving single propellers. One of the engines and its car was later removed to make room for a more powerful radio set and radio compass. The gas cells which were ten meters long, extending from main frame to main frame, were confined radially by a netting of diagonal wires on the

periphery of the ship, fastened to the longitudinal girders. The volume of the gas cell was 2,115,174 cubic feet. The entire frame work of the ship was covered with a doped fabric.

The control car was secured to the keel by struts and steel wires running through them. In the control car were the steering controls, ballast controls, hand gas valving controls and engine order telegraphs, as well as the instruments for recording and indicating altitude, speed, temperature, etc.

The *L-49* of the German Navy was used as the prototype of the *Shenandoah*, but the plans of the *L-72* (of the latest war time Zeppelin type) were also followed to some extent. While the plans of the *Shenandoah* followed those of its model very closely, rigid strength calculations were made to find all possible weaknesses in any part, and a number of minor changes were made. The design of the *Shenandoah* was referred to the National Advisory Committee for Aeronautics so that its strength and especially the method of stress calculation might be examined into. A special committee was appointed which devoted a great deal of time to the examination of the plans and specifications of the *Shenandoah* and similar airships, and held many conferences with the engineers of the Bureau of Aeronautics.

As the result of its enquiry, the committee arrived at the following conclusions:

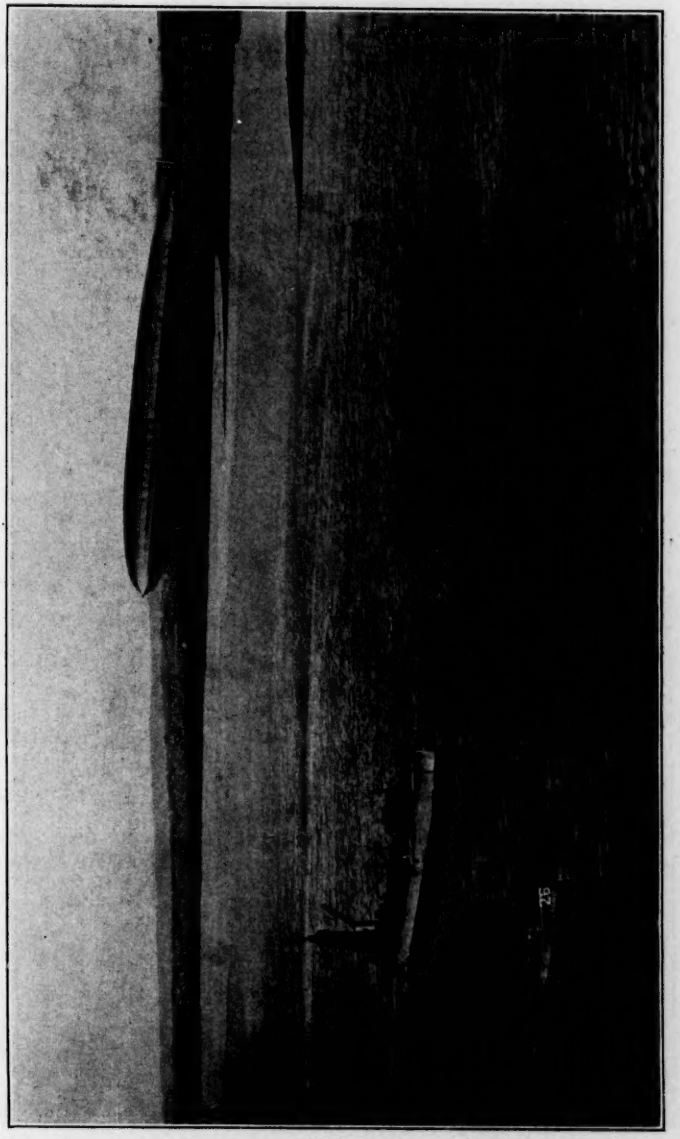
(1) The only wise policy was followed by the Bureau of Aeronautics in basing the design of the *ZR-1* (*Shenandoah*) on that of a successful airship while checking its strength by detailed computations.

(2) The German dirigible *L-49*, selected as the prototype, was more suitable than any other, as it embodied the most extended available experience with rigid airships.

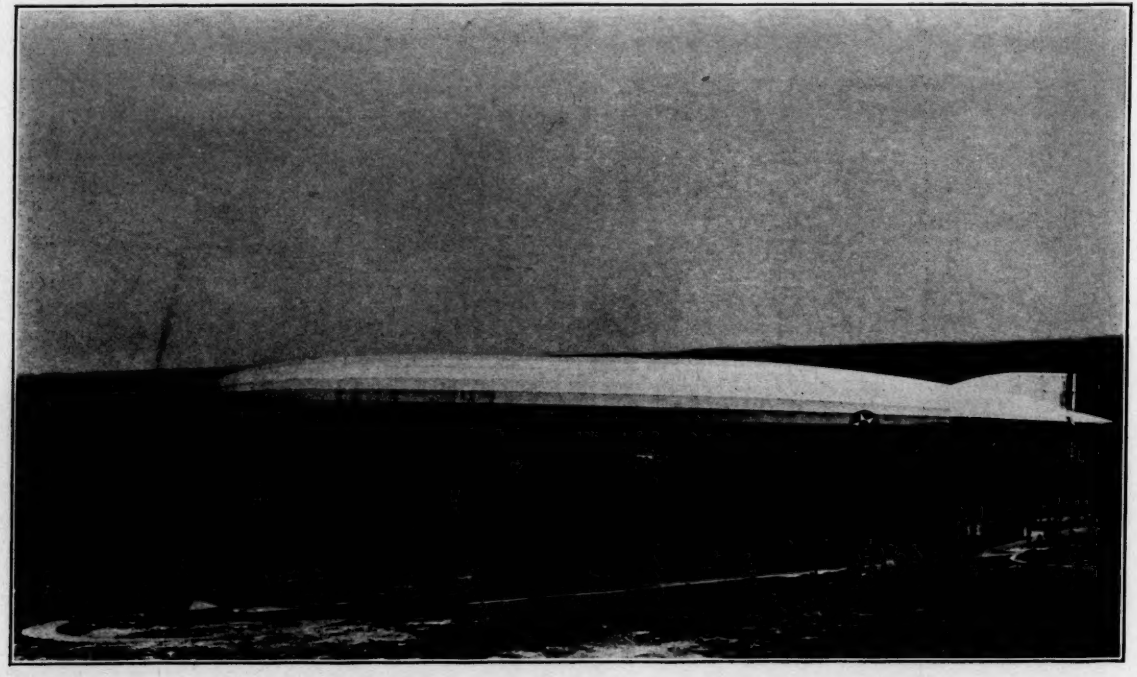
(3) The modifications made in the details were based on sound considerations, and add materially to the strength of the *ZR-1*.

(4) The external loading assumed in the calculations is more nearly correct than that used in previous airships, especially the dynamic loads which were derived from special studies made in connection with this design.

(5) The stress analyses, given in the Design Memoranda, are



"SHENANDOAH" APPROACHING U. S. S. "PATOKA," AUGUST, 1925.



U. S. S. "SHENANDOAH" AT MOORING MAST, LAKEHURST.

founded on sound principles and form a more complete treatment of the stresses in rigid airships than any hitherto published.

(6) With the modifications described in the report, the "method of bending moments" is a satisfactory method for finding the primary stresses in the longitudinal girders.

(7) The calculations for determining the stresses due to gas pressure, and the secondary and other minor stresses, are also correct within reasonable limits.

(8) The maximum unit stresses, while relatively higher than those used in steel structures, correspond to factors of safety which have been found entirely permissible in successful aircraft.

(9) The fins, rudders and elevators are fully as strong as those in the *L-49* and similar airships.

(10) The gas valves are of proper size to prevent unduly high pressures under all operating conditions.

(11) Excessive differences in air pressure, within and without the ship, are fully guarded against by ample openings in the outer envelope, with automatic covers.

(12) Owing to the careful specifications and rigid inspection, the quality of the material and workmanship is of an unusually high standard of excellence.

(13) As shown by the specimen tensile tests, the duralumin used is of a very uniform grade and has the specified strength and ductility.

(14) The numerous full sized girder tests indicate a very uniform and entirely satisfactory breaking strength.

(15) The program of trials and tests is well conceived and will give much valuable information. It should be fully carried out before the acceptance of the ship.

(16) The *ZR-1* is shown by comparative calculations to be measurably stronger than the British airship *R-38*, which failed on a trial trip, while other possible reasons for the failure, besides structural weakness, have been guarded against in the *ZR-1*.

(17) Judging the design of the *ZR-1* as a whole, the committee has been very favorably impressed by the thorough studies made by the engineers in charge and the good judgment shown through-

out in applying their results, and also the great care shown in executing the plans. It sees no reason whatever to doubt that the *ZR-1* will prove a complete success in service.

Respectfully submitted,

SPECIAL COMMITTEE ON DESIGN OF AIRSHIP *ZR-1*.

HENRY GOLDMARK, *Chairman*,
W. HOVGAARD, *Member*,
L. B. TUCKERMAN, *Member*,
MAX M. MUNK, *Member*,
W. WATTERS PAGON, *Secretary*.

The *Shenandoah* was built at the Naval Air Station, Lakehurst, New Jersey, and was launched on August 20, 1923. The first flight was made on September 4, 1923, so that the ship was about two years old when wrecked on September 3, 1925. Her statistics are tabulated below.

Shenandoah launched 2:00 p.m. 20 August, 1923.
First flight, 4 September, 1923.
Shenandoah wrecked about 0450, 3 September, 1925.

1. Number hours existence (launching to destruction)	17,871
2. Number hours inflated	12,513
3. Number hours deflated	5,358
4. Number hours outside hangar, hours, minutes	1331—05
5. Number hours inflated and in hangar	11,182
6. Number hours underway in flight	740
7. Number flights	57
8. Average length per flight, hours	13
9. Hours at shore masts, hours, minutes	439—47
10. Hours at Patoka mast, hours, minutes	70—52
11. Moorings to shore masts	9
12. Moorings to Patoka mast	7
13. Times walked to shore masts	15
14. Times walked out of hangar	35
15. Times walked into hangar	34
16. Times taken off from ground	29
17. Times landed on ground	40
18. Times taken off from masts	27
19. Average number persons carried	40
20. Average amount fuel carried, pounds	12,800
21. Average amount water ballast carried, pounds	5,363

22. Average highest flying altitude, feet	3,175
23. Highest altitude reached, feet	8,800
24. Average percentage fullness at beginning of flights	88
25. Average purity of helium, per cent	94.6
26. Average amount lubricating oil carried, pounds	640
27. Average amount reserve radiator water carried, pounds	948
28. Total ground miles covered	25,835
29. Total moorings to masts	31

By the Navy Regulations, after the wreck of a vessel the Commanding Officer is required to make a report of the circumstances to the Secretary of the Navy as soon as possible. Lieut.-Comdr. Rosendahl succeeded to command upon the death of Lieut.-Comdr. Lansdowne and Lieut.-Comdr. Hancock. His report follows:

LAKEHURST, NEW JERSEY,
21 September, 1925.

From: Lieutenant Commander Charles E. ROSENDAHL, *U.S.N.*,
Senior Surviving Line Officer, U.S.S. *Shenandoah.*

To: The Secretary of the Navy.

Via: Commanding Officer, Naval Air Station, Lakehurst, N. J.

Subject: Loss of the *Shenandoah.*

Reference: (*a*) Article 841 Navy Regulations, 1920.

In accordance with reference (*a*), the following report of the loss of the U.S.S. *Shenandoah* on the morning of 3 September, 1925, in Noble County, Ohio, is submitted.

At noon (plus 5 time), 31 August, 1925, the *Shenandoah* was reported to the Commanding Officer as in all respects ready for flight. In preparation for the so-called Midwest Flight, the ship was undocked and walked out of the hangar at 1726 on Tuesday, 1 September, 1925, and after being walked across the field was secured to the mast at 1816. The average percentage of inflation was 91, and there was on board 14,544 pounds of fuel and 9075 pounds of water ballast, 600 pounds of ballast having been dropped during the mooring operation. While riding to the mast no unusual event occurred except that a chain mooring drag was tried out at frame 40 and functioned fairly satisfactorily. Additional fuel was taken on board to bring the total to 16,620 pounds. Eight hundred and sixty pounds of lubricating oil and 948 pounds

of reserve radiator water were also on board. Pursuant to Navy Department order 29492-266:51 Op-39-G-MZ of 12 August, 1925, modified by Navy Department order Op-38-C-MV 29452-66:51 of 15 August, 1925, at 1452 (plus 5 time) Wednesday, 2 September, 1925, the *Shenandoah* cast off from the Lakehurst mast and the course was set for Philadelphia at an air speed of 38 knots as the beginning of the Midwest Flight. The total number of persons on board was 43. Water ballast remaining after taking the air was 6850 pounds. The altitude was set at 2000 feet. Everything in the ship was functioning normally. This air speed was maintained, as it gave a ground speed of at least 30 miles per hour as required by the *Shenandoah's* schedule.

At 3.56 a.m. Thursday, September 3, the ship was between Cambridge and Byesville, Ohio, the course having been set from Wheeling for Zanesville, Ohio. At 0400 the clock was set back one hour to Zone Description plus 6 time. At 0310 (plus 6 time) Central Standard Time, the ship's position was over Byesville. From this time until the disaster occurred the ship made practically no headway. Several changes of course were made, but these, in general, resulted only in changing the drift and leeway made. At 0435, as Navigator, I cut in the position as one of two towns — Buffalo or Pleasantville. Cambridge and Byesville were still in sight. No particular efforts were made to locate the position definitely during this time, as all hands were engrossed in an effort to save the ship and it would have been an easy matter to locate the position and resume the course to Zanesville had the ship escaped the weather.

The passage over the mountains had been uneventful except that it was very smooth. At no time was the ship pressed for altitude, as the ship's pressure height was approximately 3800 feet and by flying at altitudes up to 3500 feet there was sufficient altitude increment to handle the ship easily. Having crossed the mountains and arrived at Connellsville the altitude was changed to 3000 feet and shortly thereafter to 2500 feet.

At midnight I was relieved of the Navigator's watch by Lieutenant Commander Hancock, now dead, and I turned in in my bunk in the keel. At about 0330 (plus 6 time) I was called to relieve Lieutenant Commander Hancock and did so very soon

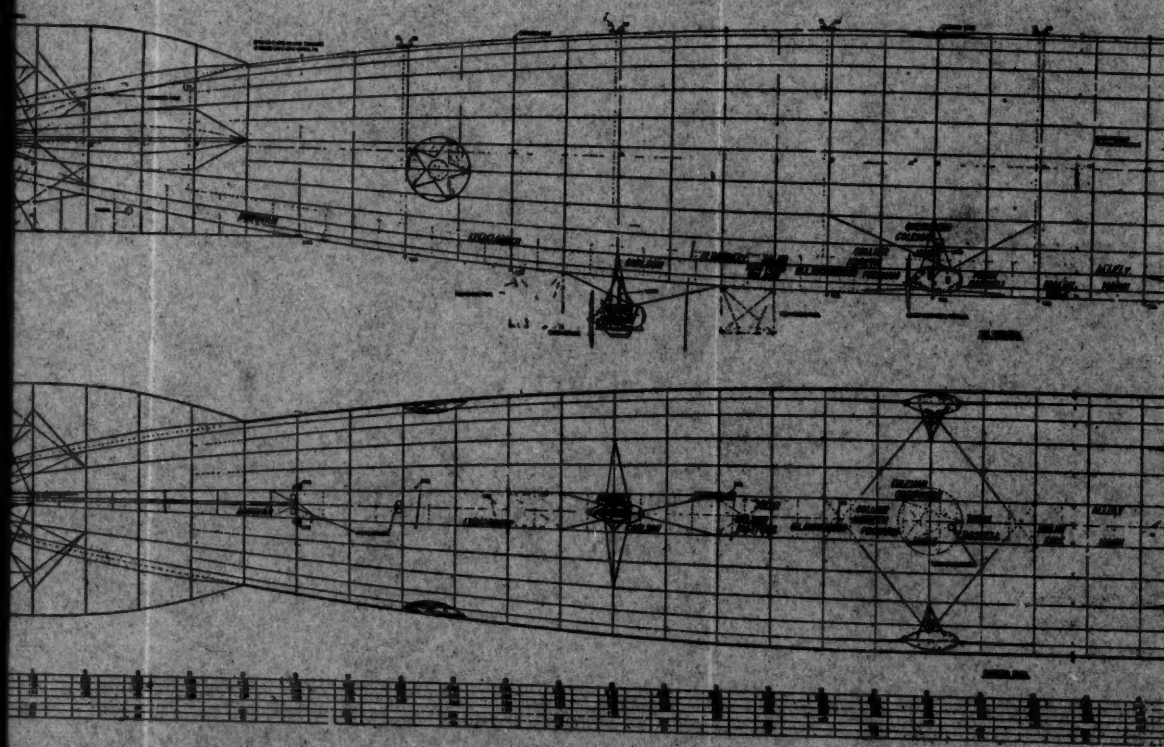
thereafter. I was told upon taking over the watch that the ship had been taken off her course to avoid a thunderstorm and that our ground speed had been materially reduced during the previous watch. The course was then 180 degrees psc with a 33 degree drift to the left; this course having been set to avoid the storm, altitude 2500 feet, air speed 38 knots. The ship was then over Byesville. The ground speed was practically zero. A severe electrical display was in evidence to the northward and eastward, but at a considerable distance. This was the storm the ship had just avoided on the mid watch. Since we had apparently successfully avoided this storm, the Captain gave orders to change course somewhat to the right (west) in order to resume a more general direction to our objective — Zanesville. At 0345 the course was changed to 240 degrees psc, variation 2 degrees west, deviation zero, drift 15 degrees left, which gave us a true course of 223. The electrical display extended from slightly forward of the starboard beam aft to a little on the starboard quarter, and heavy dark clouds extended from there to a few points on the port quarter. At 0350 the course was changed 10 degrees to the right or to 250 degrees psc. Since we were making practically no ground speed at 2500 feet, the Captain ordered the altitude changed to 2100 feet by the elevatorman's altimeter. At 0330 engines 1, 2 and 3 were speeded up to standard speed for a short time only. At 0350 the drift was about 20 degrees to the right. At 0405 the drift was again measured and found to be about 45 degrees to the right, the ship still making practically no headway. At 0408 engines 1, 2 and 3 were rung up to standard speed, while 4 and 5 remained at two-thirds. The drift was then estimated at 55 degrees to the right. Cambridge was slightly abaft the starboard beam and distant about 8 miles. The next and last position cut in was that spoken of above as at 0435.

When I came on for this last watch there were present in the control car Captain Lansdowne, Lieutenant-Commander Hancock, who remained in the car after I had relieved him; Lieutenant Houghton, Officer-of-the-Deck; Lieutenant Anderson, Aerological Officer; Colonel Hall, Air Service, U.S.A.; Joffrey, R.T., Aviation Rigger 1st class, U.S.N.; Rudderman, Allen, E.P., Aviation Chief Rigger, U.S.N.; Elevatorman, Schnitzer, Geo. C.,

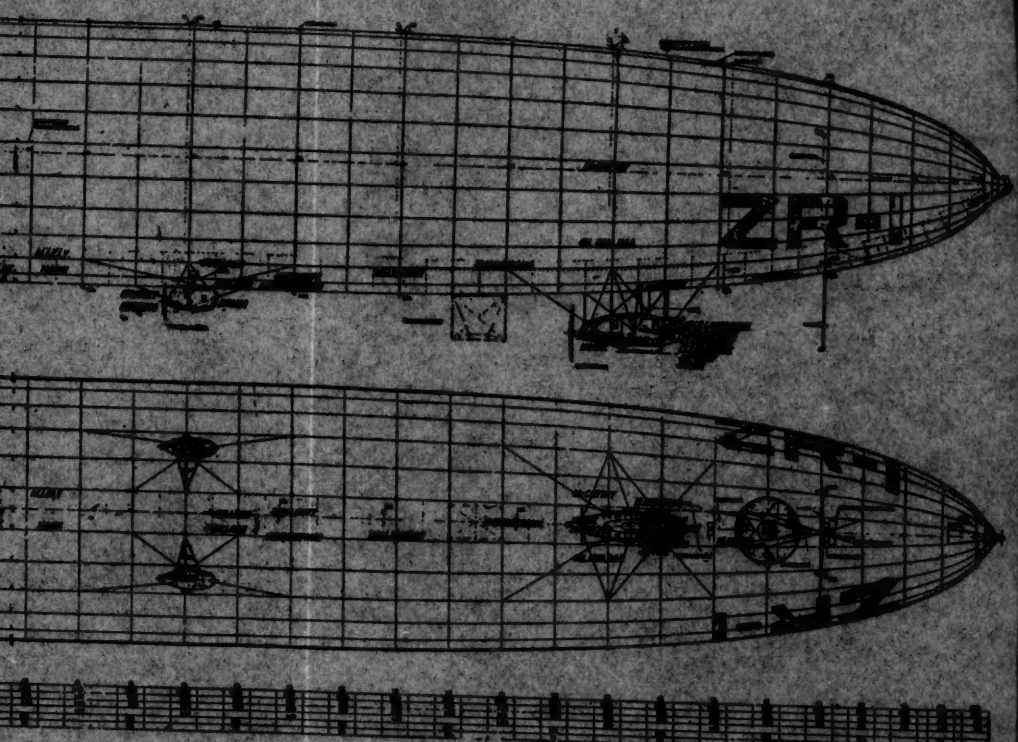
Chief Radioman, U.S.N., and the radio generator mechanic, who I learned later was Moore, James A., Jr., Aviation Machinist's Mate 1st class, U.S.N. Captain Lansdowne was watching the weather and particularly the storm that the ship had just avoided. The moon showed itself ahead occasionally with a rather dull light. During my drift observations taken out of the control car window, I observed on the starboard bow a streaky cloud forming, and, although it did not seem threatening, I turned to call Lieutenant Anderson's attention to it. However, he was sitting in the after end of the control car and watching out of the window to starboard, so that I was sure that he saw this cloud. Shortly thereafter the Captain came over to the starboard side and it was apparent to him that this new cloud was either coming towards us or building up very rapidly. About this time the Elevatorman called out that the ship was rising. He was told to check her and put on sufficient down elevator to check her. However, he kept reporting that the ship was rising and that he could not check her. The engine speed was increased and the angle of inclination of the ship was increased to a very considerable inclination down by the nose in the effort to drive the ship down against the vertical current which was lifting the ship very rapidly. The angle exceeded that which could be registered on the Elevatorman's inclinometer, and several times I turned my flashlight on the large inclinometer which was mounted on the chart board and read the angle as 18 degrees. The Captain ordered that this angle be not exceeded, as there was danger of stalling the ship. The ship continued to rise and reached an altitude somewhere near our pressure height, and there the rate of ascent was checked materially and it was believed that we had checked the ship's ascent and would begin to descend. However, this did not materialize and the ship began rising very rapidly, the Elevatorman continuing to call out the rate of ascent at frequent intervals. This rate of ascent got to be over 2 meters per second at times. Several changes of course were made by the Captain's direct order to the Rudderman, all of which were to change our course to the southward. Due to the motion of the ship, the Rudderman seemed to be having a very difficult time making these changes. At the second rapid phase of the rise it was considered essential to assist

in the delivery of gas from the gas cells through the automatic valves by use of the hand or maneuvering valves. Lieutenant Houghton was ordered to valve by hand and I was told to time the hand valving as was customary, so that we could calculate the amount of buoyancy lost by valving. The hand valves were operated five minutes, timed accurately by stop watch. The Captain then gave the order to discontinue the hand valving, as it was believed that this valving would assist materially in checking the rise. The rise continued for a short while thereafter and then came to a stop. In order to check as early as possible the excess momentum which the ship would probably gather in descending after its rapid rise, the Captain gave orders to dump ballast. At least two officers reached for the toggles at once. At that time, due to the water thus far recovered from the engines, the total amount of water ballast available had accumulated to 9600 pounds. As learned afterwards, practically all of this water ballast except about 1600 pounds or a little more, was dumped from the control car. The ship began a very rapid descent. I believe that this descent lasted but a matter of approximately two minutes, but its rate was such that I experienced a very decidedly unpleasant feeling in my ear drums. I could not observe at all times the instruments on the instrument board, as the Captain and the Executive Officer and others were there practically continuously observing the fluctuation of the instruments. Word was sent up into the keel by at least two officers, Lieutenant Houghton and Lieutenant Anderson, to stand by to slip fuel tanks as our only salvation lay in checking the ship's descent by such ballast. Both of these officers went into the keel and returned to the control car. Lieutenant Lawrence, although off watch and asleep, awakened by the unusual conditions in the ship, came down into the control car to see if he could be of assistance. During the very rapid ascent No. 2 engine rang up on the telegraph that he had stopped on account of high temperature, and a few minutes later No. 1 car rang up indicating the same thing. The other three engines continued in operation. After a very short period following the checking of the descent, the ship leveled off. This lasted only for a short while, however, and then the ship began to rise rapidly. I believe that this rate of ascent exceeded that of the first rapid

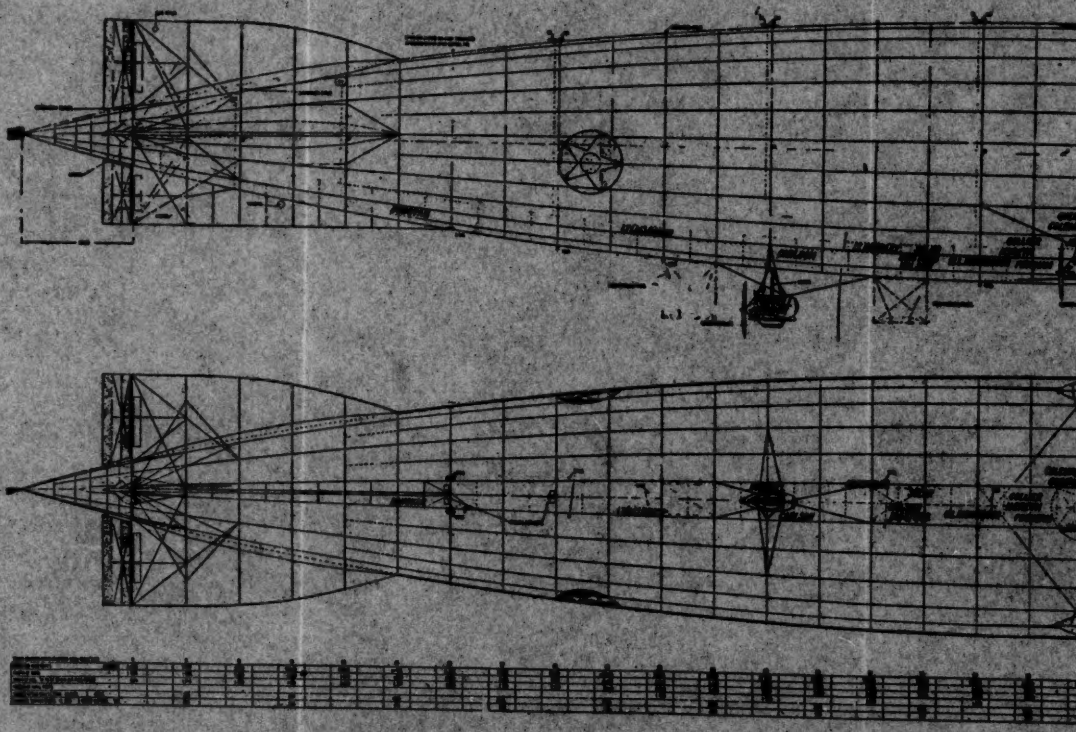
rise. It was apparent to the Captain and all of us in the control car that once we began the next descent that the ship could be saved from a probable crash only by slipping a number of fuel tanks. The Captain asked me to go up into the keel and see that they were actually ready to slip fuel. As I stepped upon the ladder leading into the keel the ship took a very sudden upward inclination which seemed to me very much the same condition that exists in a plane upon the beginning of a loop. The angle was greater than any I had ever experienced in an airship. As I was climbing the ladder I heard struts snapping and it seemed as though the control car lagged behind the ascent of the bow. However, I did not think at that time that the control car would leave the ship, as there were suspension wires inside the struts, and I thought it was merely the twisting motion that had resulted in some of the struts being snapped, as there had been more or less rolling motion of the ship practically the entire time of our rapid vertical movement. The amount of the roll I do not know, as there was no instrument for recording it. In going up the ladder the angle and the motion of the ship was such that my flashlight which was in my rear pocket caught on the hatch and tore off the pocket completely. I worked my way in the dark up into the keel and started aft to go amidships where I would find the largest number of tanks available for slipping and where I expected to find the keel officer and other members of the crew. I did not get past frame 150 when there was a terrific crashing of metal and a combination of noises hard to describe. The bottom keel panel was pulled out and a number of transverses in the keel between M and M' were severed. This was undoubtedly caused by the control car carrying away from the ship. I cannot recall how soon thereafter or whether it occurred simultaneously, but I remember that my next vision was that of the after part of the ship floating rapidly away and down. This was preceded and accompanied by a crashing sound of metal such as one would hear if a large bunch of lattices were thrown into a pile and jumped on violently. Aviation Chief Machinist's Mate Coleman, who was seated on his car, No. 2, pouring water into the radiator, states that he saw No. 4 car, which was directly ahead of him, swing under the ship from its fixed position. This was before the ship broke in two, and



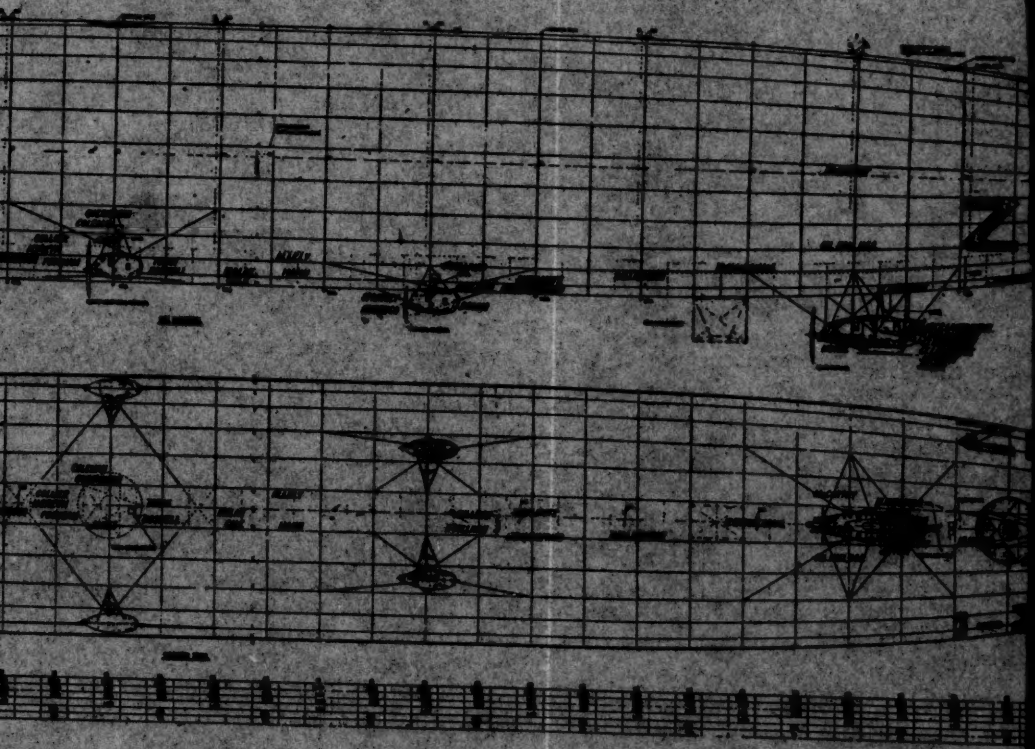
LOCATION OF PERSONNEL ON "SHENANDOAH" AT TIME



AT TIME OF HER LOSS.



LOCATION OF PERSONNEL ON "



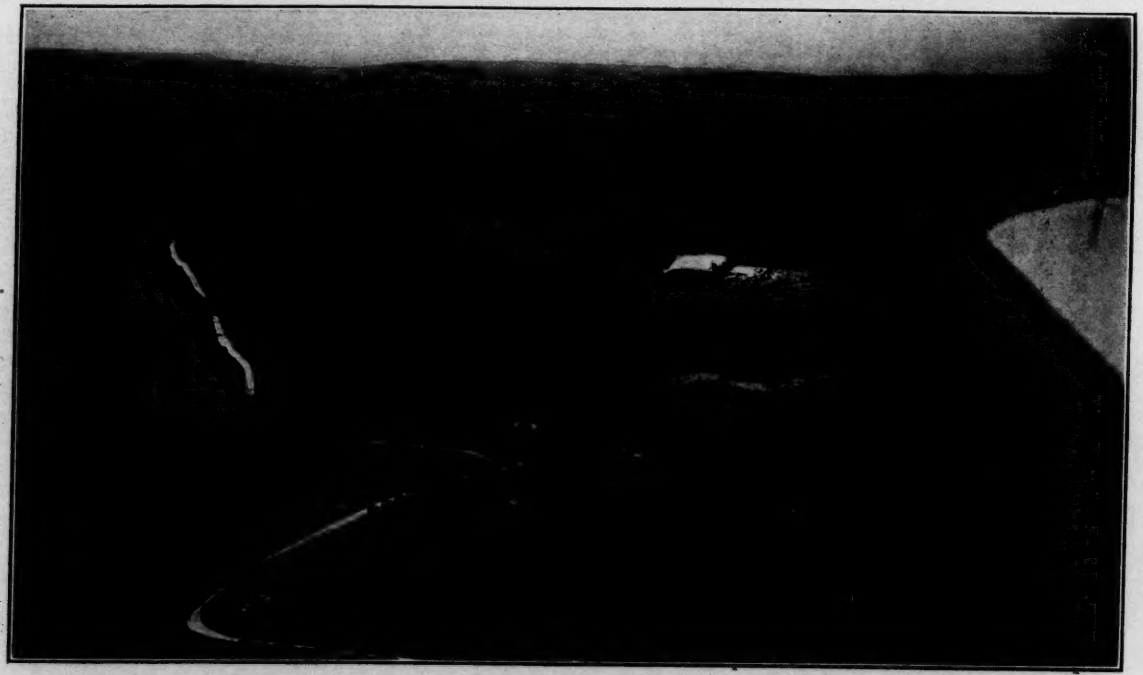
NEL ON "SHENANDOAH" AT TIME OF HER LOSS.

believing that his car might suffer the same fate, he called to the mechanic who was inside the car to come up with him inside the ship. I realized at once that my section of the ship was floating, and looking forward I could see the hole through the keel that had been exposed when the control car carried away. Some time shortly thereafter I heard a terrific crash on the ground, more of the nature of a thud. It occurred to me at once that this was the control car striking the ground. I still saw the after portion of the ship settling to the ground and I saw it suddenly arrested in its downward flight, and I believed it to be striking the ground. The forward section of the ship rose until we were almost out of sight of the ground and at the same time began spinning in a horizontal plane. My first impulse was that I was alone and although the section was floating, I pictured that I would have a very difficult time landing it safely owing to the fact that I could not move freely due to the condition of the keel walkway. However, I shouted toward both ends of the ship and immediately received a reply from both ends. I found that aft of me were Chief Machinist Halliburton and Machinist's Mate Shevlowitz. Chief Machinist Halliburton was in the Officers' Quarters at approximately frame 135, the ship having broken in two slightly abaft there at about frame 130. Forward of me was Lieutenant Mayer, who answered up and told me that with him were Lieutenant Anderson, Colonel Hall and Aviation Chief Rigger McCarthy. It so happened that our weights were fairly well distributed so that our section of the ship remained fairly well on an even keel. Lieutenant Mayer and I carried on a conversation and I learned that he had access to a valve control wire and as a result could valve from one of the forward cells. He also found that there remained some 1600 pounds of water ballast in one of the forward bags. At the other end of the ship we had two nests of fuel tanks, but we did not have pliers to slip them. However, we were able to drain their contents from their bottom connections. From this time on it was simply a matter of free ballooning. Lieutenant Mayer and I passed the word to each other in regard to valving and ballasting. Colonel Hall was located in the vicinity of this valve control wire and performed the actual valving as we deemed necessary. Lieutenant Anderson, as the control car left the ship,

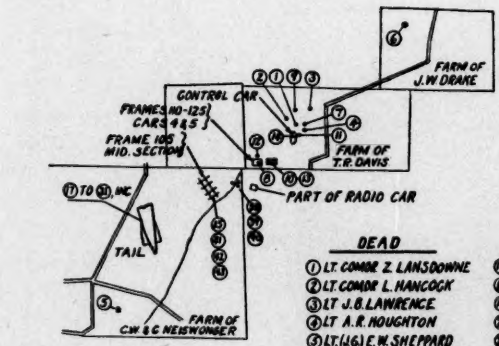
had been left astride the keel walkway and only by holding with both hands on wires in the vicinity was he able to stay in the ship. After it was apparent that we had this section of the ship under control, Lieutenant Mayer got a manila line and passed it to Lieutenant Anderson, who put it under his arms. Lieutenant Mayer then hauled Lieutenant Anderson up into a position of safety in the structure. Shortly after the break occurred we were in a heavy rainstorm, and later during our flight we were again in a lighter rain. Practically all during this time our section of the ship was spinning in a horizontal plane rapid enough to cause dizziness, and it was necessary for all of us to hold on by hand. We realized at all times that we had our part of the ship under control statically. It was found that the trail ropes and trail rope hatches were intact and the trail ropes were dropped as for a landing. The first time we descended to an altitude from which we expected to make a landing we were making a very high ground speed which I estimated to be at least 25 miles per hour. We, therefore, did not land, but allowed the ship to continue over the ground. We struck a clump of trees on a hillside. One large tree projecting into the ship at about K' longitudinal ripped through the outer cover practically down the length of the ship. When we gained altitude after this impact we called out to see if anyone was missing. McCarthy did not answer up and we surmised that he must have been knocked out of the ship. We continued to drag for probably one-half mile more when we saw we would bring up against a hill between a farm house and a barn. It was then decided to valve down as much as possible and effect a landing there. Those of us who were able climbed into the rigging and cut open the gas cells as high as we could reach so as to insure our staying on the ground. The trail ropes and a mooring wire which had also been dropped succeeded in catching in trees and fences and pulled the nose down to the ground. Those in the forward section of the ship immediately jumped out and did what they could with the lines and wire to secure the ship to the ground. The after end of the ship was still about 20 feet above the ground, so that it was dangerous for us to jump. We had the people from the forward end locate a light manila line which they threw up into the ship, where we secured it over the apex girder. Chief Machinist Halli-

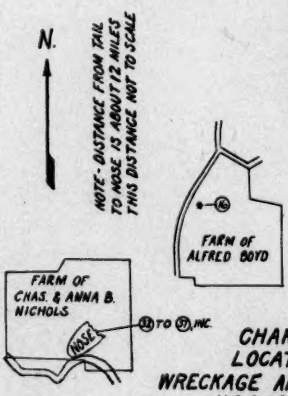
burton, Machinist's Mate Shevlowitz and I then descended to the ground by this line. This section of the ship, due to the fact that the sun was getting stronger, seemed to be gaining buoyancy and was also being thrashed about by the surface wind. I feared that it would either destroy the house near which we had landed or might even take the air again, so I called for pistols and shotguns from the people who were then gathering and we shot holes through the top of the cells to deflate them. Due to the condition of the ship and to her pounding motion on the ground it would have been extremely dangerous for anyone to have gone back into the ship until she was deflated and anchored. The time that I landed from this section of the ship was, by my wrist watch, 0645 plus 5 time. The nearest town was Sharon, Ohio. When I landed I had no knowledge that there were survivors from the other portion of the ship. It developed, however, that in addition to the seven who were in the forward section of the ship, there were others who had landed safely in the after section, and it was also learned through the natives who were gathered that there were a number of dead and perhaps one or two injured. When it was apparent that the forward section of the ship would remain where it landed, I decided to find the nearest telephone and get off a dispatch to the Department. Lieutenant Bauch, who had landed in the after section of the ship, had stationed lookouts to follow the forward section in its flight as a free balloon and had compiled a list of survivors, dead and injured as known to him. When he saw that the forward section had landed, two CPO'S were dispatched with this list to the forward section to learn of survivors there and of the general condition. This list which reached me roughly within an hour after I landed was the basis of accounting for the entire ship's company. There was still doubt as to who were injured and who were dead, and several parties of volunteers from the citizens of the vicinity set out to look for McCarthy and look for the dead. With this preliminary list I reached a telephone and got the telegraph office at Caldwell, Ohio. Lieutenant Bauch was there preparing to send a dispatch containing such information as he had. Combining his information with mine, I dictated over the telephone a message to be sent at once. For some reason this dispatch did not get through the telegraph lines, and the first

official dispatch which I sent to the Navy Department was that which began QUOTE following definite information, etc., UNQUOTE. When the ship broke in two parts, the break occuring at approximately frame 130, the after part of the ship having the five power cars was statically heavy and crashed without any semblance of free ballooning, although efforts were made to lighten her as much as possible by dropping of fuel tanks and other weights. The reconstructed history of the fall of this after section indicates that the section from frame 130 to frame 90 broke off shortly before reaching the ground. This so lightened the tail section that immediately upon its first crash it arose slightly and dragged about one-half mile from where the amidships section crashed. The latter remained where it hit, a jumbled mass of metal, fabric and the two power cars 4 and 5. From this amidships section the survivors were Chief Gunner Raymond Cole, Aviation Chief Rigger L. E. Allely, and Ship's Cook 1st class J. J. Hahn. Gunner Cole suffered slight injuries when landing and was taken to the Marietta Hospital at Marietta, Ohio. The bodies of Aviation Chief Machinist's Mate Broom, Aviation Machinist's Mate Spratley and Aviation Machinist's Mate Mazzuco were found with or near the amidship section, indicating that they had not left the immediate vicinity of their stations in power cars 4 and 5. The tail section from frame 90 aft after striking the ground on a hillside left No. 1 car on its side of this hill and pivoting approximately about its forward end swung through an arc of about 180 degrees over the ground and came to rest, power cars 2 and 3 remaining attached to the structure. The survivors of this section were as follows: Lieutenant C. E. Bauch, Lieutenant T. B. Hendley, Aviation Rigger 1st class Boswell, Aviation Chief Rigger Carlson, Aviation Chief Machinist's Mate Coleman, Aviation Chief Rigger Collier, Chief Boatswain's Mate Donovan, Aviation Machinist's Mate Hereth, Aviation Chief Machinist's Mate Johnson, Aviation Machinist's Mate 1st class Jones, Aviation Pilot Masters, Aviation Pilot Peckham, Aviation Machinist's Mate 1st class Quernheim, Aviation Machinist's Mate 2nd class Malak, Aviation Chief Machinist's Mate Russell, Aviation Machinist's Mate 1st class Solar, Aviation Pilot Tobin, Yeoman Richardson Wilson and Mr. W. L.



AERIAL VIEW OF VICINITY OF WRECK NEAR AVA, OHIO.
Stern Section, Engine Car No. 1; Wreckage of Section 110—130 and Control Car May be Seen.
Note Also Wreckage in Trees as Ship Dragged over Hillside.




N.
NOTE- DISTANCE FROM TAIL TO NOSE IS ABOUT 12 MILES THIS DISTANCE NOT TO SCALE
FARM OF ALFRED BOYD
FARM OF CHAS. & ANNA B. NICHOLS
NOSE
TO (37), INC.

DEAD

(1) LT. COMDR Z. LANSDOWNE
(2) LT. COMDR L. HANCOCK
(3) LT J.B. LAWRENCE
(4) LT A.R. HOUGHTON
(5) LT. (J.G.) E.W. SHEPPARD
(6) CULLINAN, A.P.
(7) ALLEN, A.C.R
(8) BROOM, A.C.M.M
(9) SCHNITZER, C.R.M
(10) O'SULLIVAN, A.M.M. 1/c
(11) JOFFRAY, A.R 1/c
(12) MAZZUCO, A.M.M 1/c
(13) SPRATLEY, A.M.M 1/c
(14) MOORE, A.M.M 1/c

INJURED

(15) R.COLE, GUNNER
(16) Mc CARTHY, A.C.R

UNINJURED

(17) LT HENLEY
(18) LT. BAUCH
(19) CARLSON
(20) WILSON
(21) TOBIN
(22) PECKHAM
(23) RUSSELL
(24) HERETH
(25) JOHNSON
(26) COLLIER
(27) DONOVAN
(28) MASTERS
(29) SOLAR
(30) BOSWELL
(31) MR RICHARDSON
(32) LT. COMDR. ROSENDAHL
(33) LT. ANDERSON
(34) LT MAYER
(35) COL. HALL
(36) CH MACH HALLIBURTON
(37) SHEVLOWITZ
(38) QUERNHIEM
(39) COLEMAN
(40) JONES
(41) ALLELY
(42) HANN
(43) MALAK

CHART SHOWING LOCATIONS OF WRECKAGE AND PERSONNEL OF U.S.S SHENANDOAH

SCALE - 4" = 1 MILE

Richardson. In order to anchor this part of the ship, Lieutenant Bauch had the bags slashed sufficiently to release the gas. The work was then apportioned amongst the survivors of gathering the bodies of the dead, of picking up important parts to be saved and of communicating with the survivors of the forward section, as previously indicated.

The control car landed not far from the midships section, possibly 50 feet beyond, indicating that it arrived at the ground earlier than this section. The radio cabin and the body of Chief Radioman Schnitzer were found some distance from this point, indicating that the radio cabin separated from the control car during the fall. The bodies of Lieutenant Commander Lansdowne, Lieutenant Commander Hancock, Lieutenant Lawrence, Lieutenant Houghton, Aviation Chief Rigger Allen, Aviation Rigger 1st class Joffray were found in the vicinity of the control car, strung out in an order which indicated that very probably they had been thrown out of the control car individually. Aviation Machinist's Mate Moore was found in the compartment of the control car which contained the radio motor generator. The bodies of Lieutenant Sheppard, Engineer Officer; Aviation Pilot Cullinan and Aviation Machinist's Mate O'Sullivan were found at a distance from the others, indicating that they had been precipitated from the ship probably at the break, as they had all previously been engaged in servicing the power plants, particularly with radiator water for No. 2 engine and with anti-knock fuel dope on account of the high engine speeds at that altitude. Lieutenant Sheppard when found was clutching in one hand a piece of girder and in the other hand a wire that had undoubtedly given way with him during the breaking interval. Aviation Machinist's Mate Shevlowitz had been near Lieutenant Sheppard and Lieutenant Sheppard had told him to look out for himself and not to worry about him, Sheppard. After the break Shevlowitz found himself at or near frame 135, so it is quite probable that Lieutenant Sheppard was thrown through the break near frame 130. The after section landed near Ava, Ohio, about 12 miles from Sharon. Survivors in the after part of the ship say that the forward section of the ship in its flight as a free balloon made practically a complete circle of the tail section before landing.

* * *

During the period of uncertainty in the ship, preceding the breaking in two, there did not come to my attention a single incident where any person of the *Shenandoah* did not perform his duty fully, efficiently and conscientiously. At no time preceding the disaster while I was in the control car was there a single word spoken out of the ordinary conversational tone. This was particularly true in the case of Captain Lansdowne, who received direct reports from the Elevatorman that the ship was rising and that he could not check her. Captain Lansdowne in his characteristic calm manner replied to the Elevatorman that he, the Elevatorman, was doing all that he could possibly do and that was all that he, the Captain, could expect. Although the gravity of the situation was very apparent, the Captain's order to me to go into the keel and see about the fuel tanks was given absolutely calmly and in an unperturbed manner. Lieutenant Houghton, who was on watch as Officer-of-the-Deck, was particularly keen to observe every incident and assisted the Captain with advice and with efficient responses to every order given. Chief Radioman Schnitzer came into the control car only a few minutes before the ship broke, apparently oblivious to the danger of the ship, and requested from me the ship's position so that he might reply to a message that he had just received. Since there was only little change in our position from that given him about 0420, and due to the fact that there was too much going on to write him out the position, I indicated to him to go back into the radio car and never mind the answer at that time. Lieutenant Commander Hancock, although he had been relieved of the watch, and although at the time of his relief there seemed to be no immediate danger, remained in the control car to assist Captain Lansdowne as needed. Lieutenant Lawrence, sensing the dangerous position of the ship, came down into the control car, although off watch, a few minutes before the break, to render what assistance he could. Joffray, rudderman, and Allen, elevatorman, continued to carry out their duties as efficiently as was possible, with a calm " Aye, Aye, Sir," to every order given them. As previously related, Lieutenant Sheppard, assisted by other members of the engineers force, had been very active for some time before the break in their efforts to

maintain the power plants for anything that might be demanded of them. Lieutenant Mayer, immediately upon the beginning of the rise of the ship in the vertical current, rushed into the keel to attend his duties there. Lieutenant Bauch, sensing the unusual condition in the ship, although he was off watch, was up immediately pursuing his duties in the keel in the after part of the ship. Practically all members of the crew were aroused by the unusual conditions obtaining and were at their stations for some time before the disaster occurred. Ship's Cook 1st class Hahn, immediately after landing, although stunned from his sudden ejection from the ship and to some extent injured by a blow on the head, rushed to a telephone and got off a message to the Naval Air Station, Lakehurst, in which he stated the names of the survivors and of the dead of whom he was cognizant, and also stated the fact that the forward part of the ship was still in the air free ballooning. His message ventured no conjectures and was an efficient statement of fact. Lieutenant Mayer, even as he did in the breakaway of the *Shenandoah* from the mast in January, 1924, proved himself extremely calm and resourceful. His intimate knowledge of the ship was of great assistance in landing the forward section with the survivors safely. His feat of hauling Lieutenant Anderson from his perilous position on the dangling keel into safety in the structure overhead is worthy of the highest praise. Lieutenant Bauch, although his section of the ship was falling at a rate that made a crash inevitable, was, nevertheless, placing his men where they could be for the most assistance and was making every effort to lighten his section sufficiently to check its fall and free balloon. Even before the word had been received from the control car to stand by to slip fuel tanks, Lieutenants Bauch and Mayer both had made such preparation and were awaiting only the commands of execution.

* * *

Naval personnel who picked up the dead read the time at which the watches stopped. Lieutenant Lawrence's watch registered 4.45; Chief Radioman Schnitzer's read 4.47. The Navigator's comparing watch, which was not a particularly accurate timepiece and was used principally as a stop watch, read 5.51 when picked up.

C. E. ROSENDAHL.

Lieut. Bauch was in the after part of the *Shenandoah* when it broke in two.

TESTIMONY OF LIEUT. C. E. BAUCH.

On the morning of 3 September, I was relieved from watch by Lieutenant Mayer between 2400 and 0030, Eastern Standard Time. The ship had at that time crossed over the highest parts of the mountains without loss of gas or water ballast and had descended to an altitude of 3000 feet by order of the Captain. When relieved, I turned in my bunk at frame 137.5 and remained there until about 0430 Eastern Standard Time. I started to get up and at that time was told by Lieutenant J. B. Anderson that the ship was rising and would soon reach pressure height.

I arose immediately and walked aft. The ship was rising, but the gas bags were about 97 per cent full. On my way aft I started to pull the rip panels on the jam pot covers over the automatic valves at frame 120, but found Lieutenant Mayer was already between the bags removing these covers. I continued aft and instructed a keel rigger to remove the jampot covers over the automatic valves at frame 40. This was done. The jampot covers over the valves at frames 80 and 160 were taken off early in the flight when the ship left the mast. An inspection of all gas cells I passed showed that their appendices into the internal gassing manifold were untied and clear. The ship by this time had reached pressure height and the internal gassing manifold was fully inflated. I stationed men at various parts of the keel from frame 20 forward with instructions to report to me any unusual conditions of the cells. Normal condition of the cells above pressure height prevailed and repeated feeling of the cells and internal gassing manifold as I passed through the keel assured me that the gas pressure was not excessive. The inclination of the ship at this time I judged to be about 20 degrees down by the nose. On passing frame 60 when going forward in the keel, I observed No. 1 engine pounding badly and two men attempting to drain water from the reserve radiator tank in the keel to No. 1 radiator. No. 1 engine stopped soon after this. On hearing No. 1 engine stop I figured our rate of ascent would increase, so I turned and tried to climb aft, expecting we might have some trouble with the end cells

CONTROL CAR.
Landed One-quarter Mile from Stern Section.

WRECKAGE FROM FRAMES 110 — 130 (APPROXIMATELY).
Landed 50 Yards from Control Car.

(No. 1, 2 and 3). However, the rate of ascent seemed to be slower after this and within a short time I heard water in the emergency ballast bags at frame 30 drop. The ship then started descending, and I yelled to men in the keel to stand by to slip fuel tanks. After falling some distance the ship seemed to check and start rising again. Shortly after this, however, I heard the breaking of girders somewhere forward of where I was, about frame 50 — I did not know where — and then the after section of the ship fell with great velocity. This section inclined about 30 degrees up by the tail, finally seemed to check its velocity in falling and straightened out approximately 5 degrees up by the tail. All during this time I did not see outside of the keel, so I had no idea at what altitude we were. After the section straightened out I heard the lower part of it brushing through the trees and then the voices of some men calling my name. I started to walk forward in the keel, but had not gone far when the forward end of the tail section struck the ground and I was thrown out through the keel cover onto the ground. The tail section then pivoted on its forward end, swung through an arc of about 180 degrees, and then settled on the ground. After making sure there was no one in the wreckage, I cut slits in the port side of the gas cells so they would deflate themselve as the hull rolled.

Lieut. Mayer came to the ground with the forward part of the airship.

TESTIMONY OF LIEUT. R. G. MAYER (C.C.)

I relieved Lieutenant C. E. Bauch of the keel watch at 0030 on September 3rd, and was told by him that we had passed over the mountains without valving gas or dropping water and that our maximum altitude had been just under 2800 feet and that we were then flying at 3000 feet.

After making the usual round of inspection which took me into the tail surfaces, then forward into the nose, I finally entered the control car, about 0200. We were then making fair ground speed, at an altitude of 2500, and were passing over Wheeling, West Virginia, where we were greeted by the blowing of whistles. The ship was then flying at 2500, 6 degrees heavy, with from 12 to

Landed 50 Yards from Control Car.

15 degrees down elevator, making 36 knots air speed. Later on in the morning this angle eased up to 2½ to 3 degrees, with elevator angles up to 10 degrees down, and a speed of 38 knots.

About half an hour after coming on deck I noticed intermittent flash lightning and notified Lieutenant Commander Hancock, who after carefully watching it for a few minutes, requested me to awaken Lieutenant Anderson and Lieutenant Commander Lansdowne, which I did.

When they came down on deck the situation was sized up and after looking at the night weather map which had been drawn by Lieutenant Anderson in conjunction with the weather resumé received from Lakehurst, the situation was considered not serious.

During this time the ship's ground speed was becoming less due to increasing headwinds.

We arrived opposite Cambridge around 0300, when the ship's speed with reference to the ground was almost nil. Various altitudes, engine speeds and courses were tried in an effort to gain headway, with poor results.

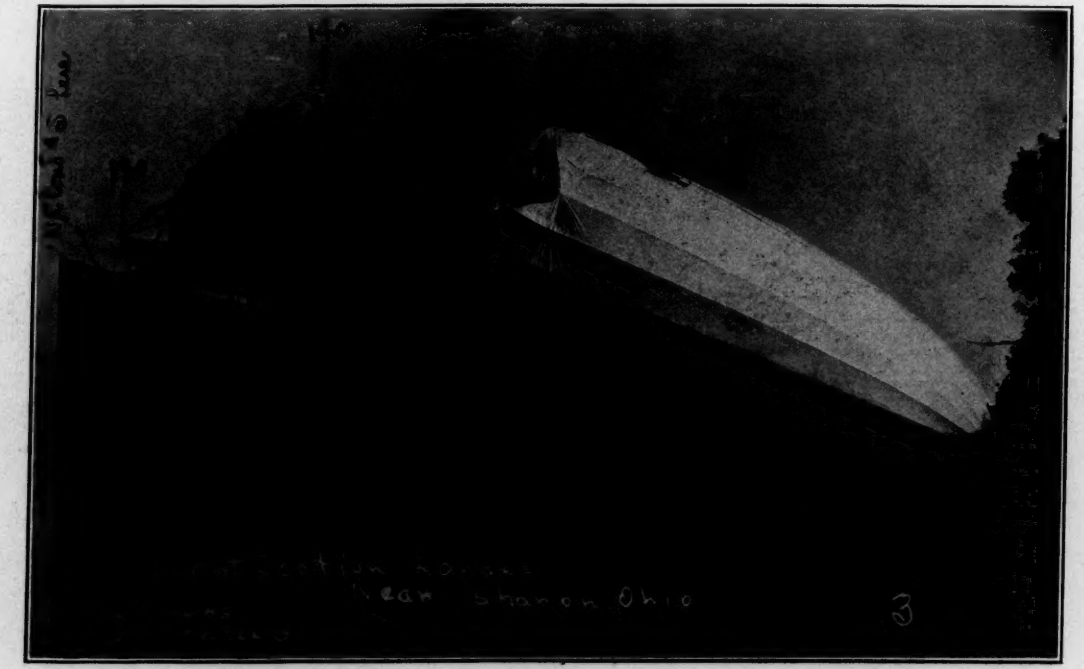
The thunder storm passing off to our stern had gained intensity and presented an incessant display of lightning. Forward a cloud formation obscuring the moon caused the Captain to change course several times until its identity had been definitely established.

During this period no air which could be termed rough had been encountered, simply very strong headwinds.

The watch had been relieved about 0400 with no change in conditions. After changing the watch the ship was dropped to 2100 feet in an effort to make ground speed. A little later the drift changed from practically none previous to this to very great angles to both port and starboard, which angles I checked twice for Lieutenant Houghton, the officer of the deck. One I remember as being 45 degrees and the other 36 degrees. However, the air was still smooth.

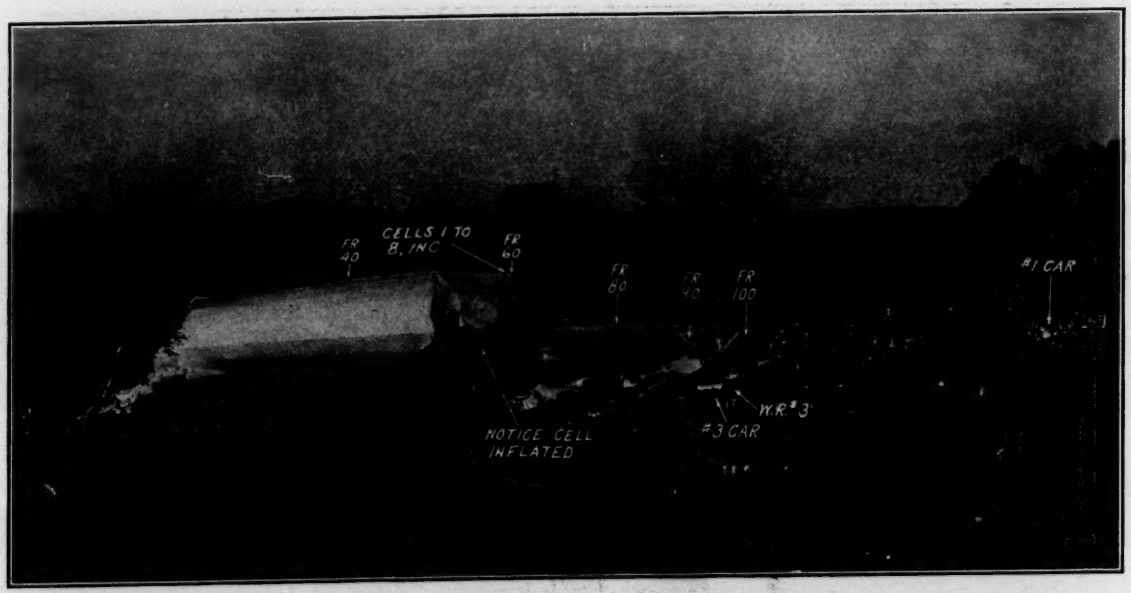
From this time on I did not check the time, as I had no occasion to look at the time piece and events merely follow in sequence; this was 0430 (E.S.T.), when the time was set back one hour.

During this interval I had made another inspection of the keel and obtained fuel data from the engineer officer so that fuel could be shifted to fill the service tanks and to change the trimming



FORWARD SECTION AFTER LANDING NEAR SHARON, OHIO; TEN MILES SOUTHEAST OF AVA, OHIO.

The Break at Frame 160 is a Secondary One Caused by Rolling Over a Ridge After Landing.



STERN SECTION AND ENGINE CAR NUMBER ONE.
Landed Near Ava, Ohio.

moment forward a little due to the slight tail heaviness caused by having the majority of the crew asleep aft.

The Captain and I were sitting on the seat on the port side of the control car aft discussing the lightning and the general weather conditions. Alongside of us sat Colonel Hall, U.S.A., and Lieutenant Anderson. While sitting looking out of the window, Allen, the elevator man, leaned over my shoulder and said to the Captain: "The ship has started to rise a meter per second and I cannot check her." We both jumped up, as it seemed so unnecessary that the ship should rise at this rate with no warning whatever. But the variometer showed such a rise. The Captain ordered the ship held down and the engines speeded up.

Prior to this, in attempting to make headway against the wind by running at standard speed, the engineer officer, Lieutenant Sheppard, had requested to dope his fuel and received permission to do so.

Finding that the ship's ascent could not be checked, I immediately went up into the keel and notified Carlson and McCarthy, who were on watch at the time, to prepare for going over pressure height; the bags were then about 96 per cent full.

In going aft to the tail the ship began to roll and pitch heavily, making it necessary to use the hand lines for steadying purposes.

In checking the ballast I noted only a small amount in the tank from the water recovery system of car No. 5. I mentioned this to Mr. Sheppard in passing him and he stated that they were running No. 5 atmospheric.

At this time all engines were running excepting No. 2, which had lost its water through a leak in the system. Men were carrying water from forward to replenish this.

Having sent McCarthy forward to watch the forward bags, I climbed up to the automatics at 120 and took the jam pots from these valves. Those at 80 and 160 having been off since leaving the mast. Carlson removed those at 40.

I then went back to frame 60 and noting that Mr. Bauch was up I went forward again, as by previous arrangement we always divided the ship amidships, he taking charge aft and I looking after the forward section.

Passing frame 90 I noted that water was still being carried to car No. 2. Here I stopped to talk with Mr. Sheppard again and passed on. At this time the ship was down by the nose to such a degree that in going to the crew's space amidships I sat on the walkway and slid down. The angle then lessened considerably. Here I found Hahn, the cook, busy securing his thermos bottles and canned goods, which had gotten adrift. I stood for a few moments talking to Allely, and we watched the altimeter go from 3500 to 4200 feet. My hand was on the inflation manifold at this time, and no great pressure was noted, which assured me that the valves were blowing satisfactorily.

We had then passed pressure height and were going up steadily. At frame 120 I passed Cullinan and Malak, who were putting tetra ethyl lead in the fuel tanks, and told Cullinan in passing that we might have to slip tanks, and to stand by when through.

Passing on to 130 I met McCarthy, who told me all was O.K. forward. Lieutenant Lawrence passed us going towards the control car. At this point I heard two wires snap in rapid succession, and I called McCarthy's attention to it. The sound was like that of a bullet ricocheting over the surface of the water. I then proceeded forward and met Lieutenant Houghton rushing up from the control car. He said the Captain wanted us to stand by to slip tanks. I told him that if our rate of ascent increased it would be necessary to use the maneuvering valves. He told me that they had just finished valving for five minutes, and that the Captain wanted to drop fuel to lighten the ship. I told him we were only awaiting an order, and he went back to the control car.

The air was getting rougher all the time as I reached the ventilating hatches forward of 170. I felt a terrific rush of cold air through them. I reached the platform at 180 and then realized that we were dropping at a terrific rate of speed, so fast that the bags were alternately cupping upward and flapping badly.

I rushed to the emergency ballast bags at frame 180 and arrived there just in time to see them emptied. I turned to go aft to 170 when the nose of the ship suddenly started up, it seemed as fast as it had been falling, the nose turning to port and the whole ship rolling up towards the starboard on the bottom. This maneuver was accompanied by a terrific vibration of the whole structure, the

sensation being akin to that experienced when putting a plane in a spin. It was necessary for me to hold fast to the keel strut to keep from being thrown into the netting on the port side of the keel.

I then heard struts cracking under my feet and my first thought was that the controls would be made inoperative and that it was necessary for me to get aft and put the emergency steering station in commission. This I started to do when I saw a man come up the ladder from the control car and start aft. At the same time I heard a terrific crashing for an instant, when a large hole opened immediately in front of me, and I heard the control car go, taking the radio car with it. The shock threw me on one knee between the two ballast bags at 170. I also saw the man who had come up from the control car throw up his hands and disappear.

The ship rose rapidly with its nose down from 4 to 6 degrees and started to spin at a very rapid rate. Through the hole in the keel I could see the tail crashing to the ground and then first realized the horror of the situation. We went up out of sight of the ground and stopped spinning when we struck an extremely heavy rain storm.

After getting myself together I went to 180 and started valving the maneuvering valve at this point until our ascent was checked. Things had gotten extremely quiet in comparison to the chaos the ship had just been through, and I thought I was all alone. Just then Lieutenant Commander Rosendahl called from somewhere aft and I answered him. I also heard a voice from the depths and I recognized it as that of Lieutenant Anderson. Noting his precarious position, I made all haste to rescue him.

While getting a line, Colonel Hall appeared and asked what he could do. I asked him to valve gas from 180, and in this way the ship started down.

Lieutenant Anderson was gotten up after some difficulty, as he could not let go his hold.

By this time the ship was so close to the ground that the two trail ropes and mooring wires were dropped. We were making considerable ground speed (over 30 miles) and our only hope was that the lines would snag a tree. We made two trial landings, but could not make the ship stick. It finally caught against a hill and by ripping and slashing all of the bags enough gas was let out to

Passing frame 90 I noted that water was still being carried to car No. 2. Here I stopped to talk with Mr. Sheppard again and passed on. At this time the ship was down by the nose to such a degree that in going to the crew's space amidships I sat on the walkway and slid down. The angle then lessened considerably. Here I found Hahn, the cook, busy securing his thermos bottles and canned goods, which had gotten adrift. I stood for a few moments talking to Allely, and we watched the altimeter go from 3500 to 4200 feet. My hand was on the inflation manifold at this time, and no great pressure was noted, which assured me that the valves were blowing satisfactorily.

We had then passed pressure height and were going up steadily. At frame 120 I passed Cullinan and Malak, who were putting tetra ethyl lead in the fuel tanks, and told Cullinan in passing that we might have to slip tanks, and to stand by when through.

Passing on to 130 I met McCarthy, who told me all was O.K. forward. Lieutenant Lawrence passed us going towards the control car. At this point I heard two wires snap in rapid succession, and I called McCarthy's attention to it. The sound was like that of a bullet ricocheting over the surface of the water. I then proceeded forward and met Lieutenant Houghton rushing up from the control car. He said the Captain wanted us to stand by to slip tanks. I told him that if our rate of ascent increased it would be necessary to use the maneuvering valves. He told me that they had just finished valving for five minutes, and that the Captain wanted to drop fuel to lighten the ship. I told him we were only awaiting an order, and he went back to the control car.

The air was getting rougher all the time as I reached the ventilating hatches forward of 170. I felt a terrific rush of cold air through them. I reached the platform at 180 and then realized that we were dropping at a terrific rate of speed, so fast that the bags were alternately cupping upward and flapping badly.

I rushed to the emergency ballast bags at frame 180 and arrived there just in time to see them emptied. I turned to go aft to 170 when the nose of the ship suddenly started up, it seemed as fast as it had been falling, the nose turning to port and the whole ship rolling up towards the starboard on the bottom. This maneuver was accompanied by a terrific vibration of the whole structure, the

sensation being akin to that experienced when putting a plane in a spin. It was necessary for me to hold fast to the keel strut to keep from being thrown into the netting on the port side of the keel.

I then heard struts cracking under my feet and my first thought was that the controls would be made inoperative and that it was necessary for me to get aft and put the emergency steering station in commission. This I started to do when I saw a man come up the ladder from the control car and start aft. At the same time I heard a terrific crashing for an instant, when a large hole opened immediately in front of me, and I heard the control car go, taking the radio car with it. The shock threw me on one knee between the two ballast bags at 170. I also saw the man who had come up from the control car throw up his hands and disappear.

The ship rose rapidly with its nose down from 4 to 6 degrees and started to spin at a very rapid rate. Through the hole in the keel I could see the tail crashing to the ground and then first realized the horror of the situation. We went up out of sight of the ground and stopped spinning when we struck an extremely heavy rain storm.

After getting myself together I went to 180 and started valving the maneuvering valve at this point until our ascent was checked. Things had gotten extremely quiet in comparison to the chaos the ship had just been through, and I thought I was all alone. Just then Lieutenant Commander Rosendahl called from somewhere aft and I answered him. I also heard a voice from the depths and I recognized it as that of Lieutenant Anderson. Noting his precarious position, I made all haste to rescue him.

While getting a line, Colonel Hall appeared and asked what he could do. I asked him to valve gas from 180, and in this way the ship started down.

Lieutenant Anderson was gotten up after some difficulty, as he could not let go his hold.

By this time the ship was so close to the ground that the two trail ropes and mooring wires were dropped. We were making considerable ground speed (over 30 miles) and our only hope was that the lines would snag a tree. We made two trial landings, but could not make the ship stick. It finally caught against a hill and by ripping and slashing all of the bags enough gas was let out to

make the ship heavy enough. I was surprised to note that it was almost seven o'clock.

After getting out we tied the front end and the windward side in an attempt to keep the ship from rolling onto the farmer's house near which we landed.

Towards noon we were able to get out all of the personal effects and left the wreck in the hands of the deputy, who stated he would guard the property.

Col. Hall also came to the ground with the forward part of the ship.

TESTIMONY OF COL. C. G. HALL, U. S. ARMY AIR SERVICE.

I arose on the morning of the disaster to the *Shenandoah* at about five o'clock, Eastern Standard Time, and went into the control car. Upon entering the car, Lieutenant Anderson, the Aerologist, called my attention to thunder clouds and lightning, and I noticed such clouds and lightning on both beams of the ship. For some time — probably half an hour or more — my attention was almost entirely taken up watching these weather conditions, to determine in my mind the situation as to whether the ship was running away from the storm area. During all of this time it appeared we were making no headway in getting away from the thunder clouds or lightning, but rather that the clouds were becoming more threatening and the lightning more vivid. Dead ahead, however, there seemed to be no storm area, and it appeared that we would eventually run into fair weather. The situation during this time gave me no great apprehension as to the safety of the ship. On coming into the control car, my attention was next drawn to the altitude at which we were flying, and I presumed we were at about 3000 feet above sea level. The first thing that caused any apprehension as to the safety of the ship was the elevator man, Allen, calling that "The ship was rising," repeatedly saying this, or "Still rising, and I can't hold her down." I noticed Captain Lansdowne leave his position at the forward starboard window in the control car and walk over to the elevator man, and observe apparently the altimeter and, possibly, the rate of climb indicated.

Some time elapsed during this situation — just how long, I cannot say; possibly a minute or two — then the elevator man called several times "She is rising very rapidly." Lieutenant Houghton then, I think at Captain Lonsdowne's order, opened the hand gas valves, the time the valves were open being called by someone, I think Lieutenant Lawrence. The time was called by the minute, and when the valves were ordered closed by Captain Lansdowne, the person taking the time said "A total of 5 minutes." At the time the valves closed I observed the altimeter, and noted an altitude of approximately 5500 feet. When the valves were closed, Captain Lansdowne called "Now, look out; we will begin to fall," and cautioned Lieutenant Houghton to stand by the ballast controls. Almost immediately the elevator man, Allen, called "The ship is falling. She is falling fast, very fast." Lieutenant Houghton at once pulled the ballast controls, holding as many controls in each hand as was possible, and kept dropping ballast until the ship had fallen to about 3000 feet, where she stopped, and no further ballast was dropped. While the ship was rising, and at about the time the order was given for gas to be valved, Captain Lansdowne ordered full speed on all engines. The signal was given on the telegraphs, I think, by Lieutenant Lawrence. During this rise of the ship the elevator man nosed the ship down at an angle, estimated by me to be about 10 degrees. During the descent of the ship it was likewise nosed up to a similar angle. From the time I entered the control car until the ship was stopped in its downward descent the air did not seem to be bumpy, but it was evident that we were running with a strong head wind. At the time the ship stopped its downward descent I took my attention from the elevator man and the altimeter, and went to the after port window to observe the ground, to see what the ship was doing. I noted a drift to port of about 45 degrees, and called Lieutenant Anderson's attention to it. I do not know whether or not he heard me, but I thought he reported to Captain Lansdowne for a change of course. I then at once went across the car to the other opposite window, and as I was about to look out, felt the car sinking under me. The impression I had was that the ship was again falling and more rapidly than at first. I looked out the window and noted that the ship was turning rapidly in a circle, and almost im-

mediately she seemed to be caught in a violent blast of wind, which appeared to throw the tail of the ship upward, at the same time turning or twisting it to the right looking aft. At exactly the time of this violent blast there was a noise as of rending girders. I heard someone — Lieutenant Lawrence, I think — say, "She's gone." I either called, or thought I called, everybody out, and started up the ladder from the control car into the body of the ship. Lieutenant Anderson called "Go up quickly, Colonel." I took but few seconds, not more than two or three, to climb the ladder, and upon stepping out on the catwalk started to run aft, so as to give room to others who might come out of the control car. I had taken but one or two steps, however, when the catwalk seemed to be pulled out from under me, and I reached out and caught a girder and drew myself up on top of the apex girder of the keel. I looked below and saw Lieutenant Anderson hanging to the catwalk, which appeared to have broken loose and was hanging freely suspended only at the end near the break of the ship and forward of where the control car was attached to the ship. While hanging by my hands I noticed that the after part of the ship had broken away, and while so suspended distinctly heard what appeared to be the falling loose of the control car. Lieutenant Mayer came to the relief of Lieutenant Anderson, and by means of a line thrown to him Lieutenant Anderson was enabled to crawl back into the nose of the ship to at least temporary safety. I climbed forward on the apex girder and then down on the decking in the nose of the ship. At Lieutenant Mayer's suggestion, I secured the one valve control wire available, and manipulated it while Lieutenants Mayer and Anderson assisted in bringing the nose of the ship to a safe landing, about ten or twelve miles from where the accident occurred.

While I was in the control car there was perfect order and discipline, and the several duties of the officers and enlisted men were preformed as calmly and efficiently as if in normal flight.

The conduct of Captain Lansdowne in encouraging the members of his crew is noteworthy. When the ship was falling rapidly, and it looked as if we would crash into the ground, Captain Lansdowne, in attempting to allay the apparent apprehension of the elevator man, Allen, said, "Allen, she's all right. We'll stop

her. She's falling fast now, but she'll slow up." I personally felt the effect of his assurances, and I am sure that its effect was felt by all members of the crew in the control car at the time of the disaster. I am sure that Captain Lansdowne with his officers and men in the control car went to their death attempting to keep control of the ship, which they had not lost until the actual breaking occurred.

In making this statement I do so with the desire that the bravery of these men be made a note of in the record of this court, and that their conduct be recorded as upholding the best traditions of the service.

Lieut. J. B. Anderson, U. S. Navy, was the aerological officer of the *Shenandoah*.

TESTIMONY OF LIEUT. ANDERSON.

Q. Lieutenant Anderson, take up this afternoon your report of the investigations concerning the weather in the vicinity of the disaster made thereto. Do you wish to read that report to the Court, or do you wish the notes, which you have prepared to accompany an oral description? A. I would like, sir, to read the major portion and to explain a few parts by reference to several maps that I have here, Figures 1, 2, 3 and 4.

Since the *Shenandoah* disaster I have studied the preceding and succeeding weather maps in an attempt to determine the character of the storm which was encountered early on the morning of September 3rd. In this study I have used all data available, such as the observations I made at the time, the data I obtained during my investigation of the storm in that locality, during the period of 19 September to 24 September, and later data which was collected by the United States Weather Bureau from their regular stations and Co-operative Observers. Much of this data is taken from reports of personal observation, obtained from those who were up at an early hour or were awakened by the storms, some of it from instruments in Weather Bureau offices, and some from the Ohio Power Company. The Weather Bureau data is considered accurate, although the reports of the Co-operative Observers must have been from personal observation, as are the Power Com-

pany's reports, since they keep close watch for electrical storms at their power stations and pass the word from station to station as storms appear anywhere along their lines. The data obtained from persons who observed the storms is subject to more error because of the fact that different persons will report the same storm as of vastly different intensities, others fail to observe either time or direction carefully and nearly all have little idea of wind strength. The reports used in this study are only those which appeared to me to be sufficiently accurate to warrant consideration.

The weather map drawn from the 8.00 a.m. 2nd September Weather Bureau reports (Fig. 1) indicated a low pressure center in Canada north of Minnesota, with a small, fairly well defined secondary center at Moorehead, Minnesota. The usual movement of storms like this is to the eastward and a gradual spreading into the Mississippi and Ohio Valleys, where local thunderstorms nearly always develop at this season. It was because of this storm that it was thought thunderstorms would be encountered on the trip west.

During the daylight September 2nd hours the small center at Moorehead shifted toward the east-southeast and was centered at Green Bay, Wisconsin, at 8 p.m., while the northern center had apparently remained almost stationary. The southern center had caused rain to develop as far east as Rochester, New York, and as far south as Detroit, Michigan, and was causing thunderstorms in Wisconsin and northern Michigan. The axis of the depression was in an east-west line, the pressure gradient was only moderate and the temperatures in the central valley were rather high. All these conditions indicated that local thunderstorms, that is, detached storms of various sizes as usually with definite areas of fair weather surrounding them, would develop in the Great Lakes district, western New York, western Pennsylvania, Ohio and Indiana during the succeeding 24 hours, the thunderstorm condition building in the west and moving east. Thunderstorms of this type are variously described, probably the best description being that of Dr. W. J. Humphreys, Meteorological Physicist of the U. S. Weather Bureau, as Cyclonic Thunderstorms. These he describes as follows: "The southeast quadrant (northeast, in the Southern Hemisphere), or less frequently, the southwest (northwest in the

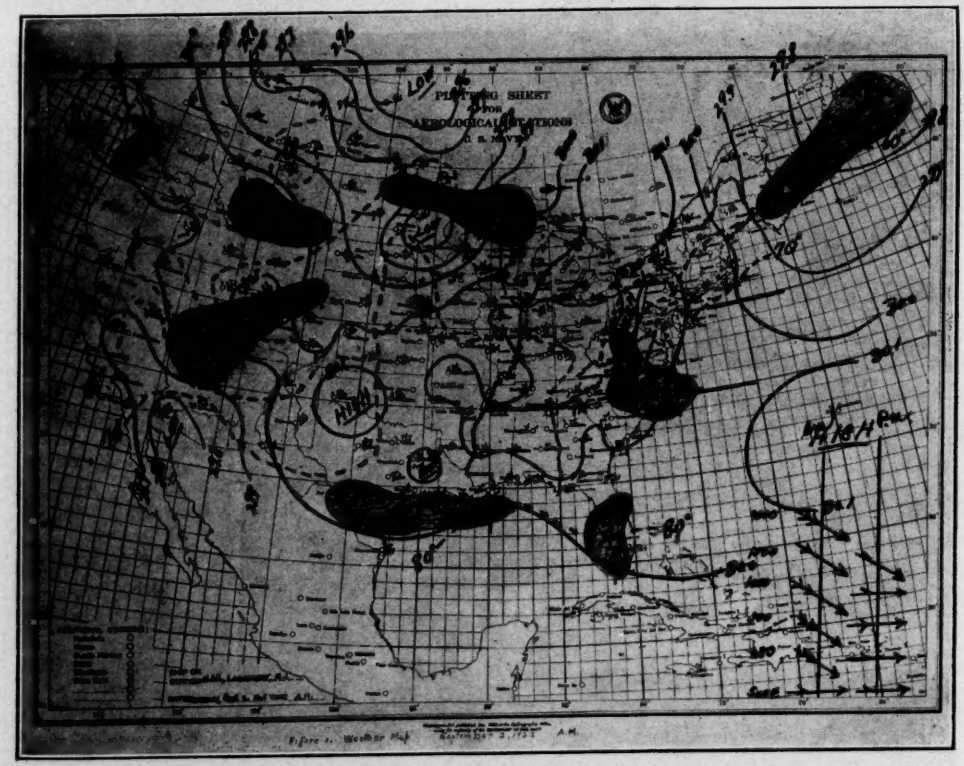


FIGURE 1.—WEATHER MAP, SEPTEMBER 2, 1925, A.M.

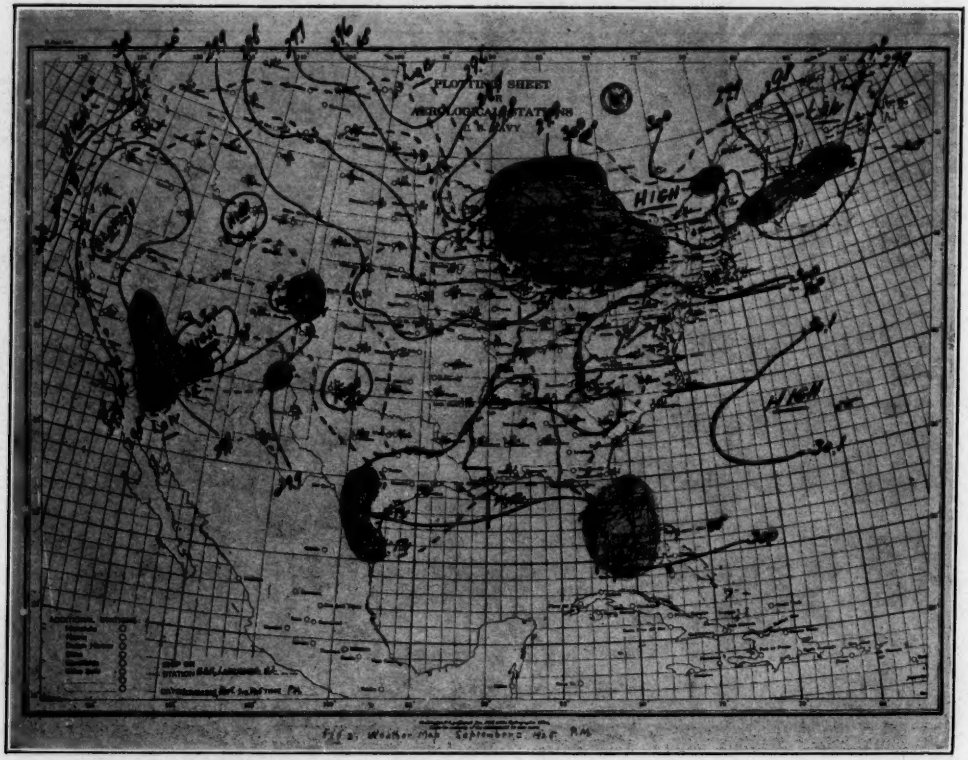


FIGURE 2.—WEATHER MAP, SEPTEMBER 2, 1925, P.M.

Southern Hemisphere), of a regularly formed low, or typical cyclonic storm.

"In this case, the temperature gradient essential to a rapid vertical convection is not produced chiefly by surface heating, as it is during the genesis of 'heat' thunderstorms, but, in great measure, results from the more or less cross directions of the under and over-currents of air, the under being directed spirally inward toward the region of lowest pressure and the over tending to follow the isobars. The surface air of the quadrant in question, therefore, normally flows from lower and warmer latitudes, while with increasing altitude the winds come more and nearly from the west, or even northwest. This crossing of the air currents, then, the lower coming from warmer sections and the upper from regions not so much warmer — possibly even colder — progressively increases the vertical temperature gradient, or rate of temperature decrease with increase of altitude, and therefore may frequently be, doubtless often finally is, the determining cause of rapid vertical convection and the formation of a thunderstorm."

During the 12-hour period following the 8 p.m. map, September 2 (Fig. 2), which was drawn on the ship slightly before midnight, the southern center of the northwestern depression continued its east-southeast movement instead of turning east, or east-northeast, as had been expected. During this movement, as it encroached on the high pressure area in the southeast, it caused the pressure gradient to increase considerably in its southeast quadrant, where the strength of the southwest winds was materially increased over a limited area. At 8 a.m. September 3 (Fig. 3), the area of stronger winds extended south from Erie, Pennsylvania, south probably as far as Wheeling, West Virginia, east a short distance beyond Pittsburgh, and north to Buffalo, New York. It was doubtless true that the area affected by the strong southwest winds, which retarded the progress of the *Shenandoah*, was no more extensive, perhaps the same general barometric conditions caused both as it moved eastward, although the two regions experienced far different conditions, since the district around Noble County, Ohio, had light to moderate surface winds, mostly easterly, while the strong wind area at 8 a.m., namely, in the vicinity of Pittsburgh, showed moderately strong southwest winds, that is,

FIGURE 2.—WEATHER MAP, SEPTEMBER 2, 1925, P.M.

similar to the apparent pressure gradient. During the night the rain area had spread rapidly southeast from the Lakes district to Norfolk, Virginia, rains being reported generally east of a line extending from Green Bay, Wisconsin, southeast to Norfolk, Virginia, including Detroit, Toledo, Parkersburg, West Virginia, and the Weather Bureau stations at Ludington, Michigan; Grand Rapids, Michigan; Cleveland, Ohio; Pittsburgh, Pennsylvania, and Elkins, West Virginia, reported thunderstorms having occurred during the night.

During the next 24 hours, the whole low pressure system moved eastward, while the southern center moved northeast to Norfolk, Virginia. Rains and thunderstorms occurred along the Atlantic seaboard from Maine to Cape Hatteras during that day.

The description of the weather maps given above shows only a rather unusual movement of a rather ordinary cyclonic storm, which was never an intense storm in the United States, nor, presumably, in Canada. As stated before, it was the type of storm which generally causes local thunderstorms of the cyclonic type, the severity of each storm depending upon the local conditions where the storm develops, but ordinarily they are not accompanied by violent surface winds. The electrical intensity varies greatly and cannot be forecast even as the storm approaches, although the size and appearance of the cloud sometimes gives some indication, since the strength of the vertical currents and turbulence there present have a certain relation to it. Many persons describe a storm as violent from the electrical intensity alone, while others demand that violent surface winds be present before calling it violent.

The quotation from Dr. Humphreys mentioned the fact that this type of storm is usually the result of winds of different directions at various levels, those near the surface from a southerly direction and those above from a westerly or northwesterly. The thunderstorms which developed in Ohio early that morning were no exception, for there was an overrunning current which, at the time, I thought was from the north, because the sky clouded from the north and after it became overcast near the ship, waves, such as form between air currents of different directions, appeared, extending mainly in an east-west direction. The actual movement of

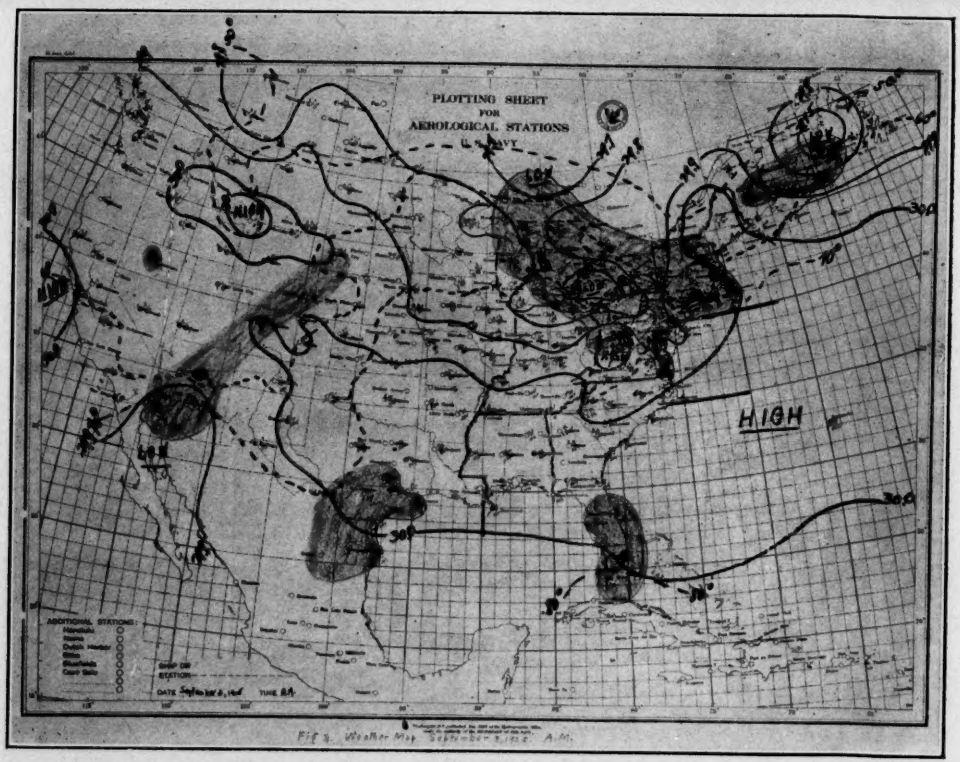


FIGURE 3.—WEATHER MAP, SEPTEMBER 3, 1925, A.M.

the clouds I could hardly determine, for it is very difficult to determine this from an airship, although I was fully convinced that the large cloud we passed under about 0330 was drifting towards the south. Further study has led me to believe that this wind was more of a north-northwest, this conclusion being derived from the fact that the storms were in nearly every case reported as moving toward the southeast, although the Akron storm apparently moved nearly south, a farmer northeast of Cambridge reported that a storm moved from the northeast over his home and on into the southwest, while at Cambridge a man observed that the surface wind was from the east and the clouds from the north.

It appears to me that this northerly, or northwesterly wind, as the case may be, overspread nearly the whole area, thereby setting up an unstable condition throughout before rains and thunderstorms began to appear, for once they began to form they appeared at great distances in remarkably short periods of time; for instance, Cambridge had a light rain about 0500, the sky was only part cloudy, seven miles southeast of Cambridge at 0520 and the sky appeared clear in the south, yet between 0530 and 0600 rain had developed at Marietta, nearly 50 miles south-southeast from Cambridge. As would be expected, thunderstorms developed in the northern part of the State before they did near Cambridge, yet there is nothing in the map to indicate that they progressed as a line squall, as has appeared in the daily press; in fact, it can definitely be said that such did not occur, for besides the evidence of the weather map, the cloud forms never showed such a condition, and, moreover, there were too many reports showing individual storms, widely separated.

The meteorological conditions in the vicinity of Ava and Cambridge were exceedingly curious, with surface winds from the east, southwest winds at 1200 feet above the surface and presumably extending at least several hundred feet below and probably at least 3000 to 5000 feet above, northerly to northwesterly, above the southwesterly. Moreover, the southwest winds were strong, doubtless between 30 and 40 miles per hour. The northerly winds were evidently of considerable velocity, since the gentleman at Cambridge who watched their movement for some time, because he had never observed different wind directions at various levels

before — said the clouds were moving rapidly toward the south. The surface wind was generally described as easterly, although a few said northeasterly, but as to strength, the opinion differed greatly, some saying it was only a moderate wind, while others said it was very strong, indicating its various effects, such as the overturning of a light wicker porch chair in Cambridge — (yet this observer called the wind moderately strong) — the waving of trees, and the slamming of doors. According to one individual in Ava, the winds were very strong, for when asked " Was the wind blowing hard?" replied " Well, I should say it was. The window shades were a-flopping." Yet a farmer about one and a half miles distant said he neither closed his doors nor windows.

It appears that there was an inversion some distance above the earth, for winds with the strength of that which retarded the *Shenandoah* seldom occur so near the earth, unless there is an appreciable surface wind in nearly the same direction, unless an inversion occurs. Moreover, the night had been clear and the winds light, an ideal condition for an inversion to occur. If such occurred, as seems probable, then it is not difficult to understand the strong southwest winds above, which were bringing warmer air into that district all the while, as I believe the thermograph record shows, blowing freely over the surface air. If this were the case, the temperature gradient, and therefore the instability in the upper air, were greater than would be indicated by comparing the ship's thermograph record with surface temperatures, and constantly increasing, for all the while the incoming northerly, or northwesterly, wind was, presumably, bringing in colder air. Once this instability began to give way, by the formation of thunderstorms and squalls, changes in barometric pressure were bound to occur, which might have caused a reactionary pressure gradient in the cooler air below, which would have caused the easterly surface current at Ava, Cambridge, and nearby localities; but how this operation would take place I am at a loss to know.

Whatever the explanation of the surface condition, there are two facts that appear to me quite certain: first, that the most intense part of the storm near Ava was confined to the upper air, and that it probably was the greatest at, or above, the cloud level, for several persons commented on the boiling of the clouds. One lady

in Ava, who watched the ship until it passed out of her view behind a hilltop, about half a mile away, only a few minutes before she saw the forward part of the ship drifting south, said that directly over the ship was a cloud in such commotion that she called attention of her husband to the way it was boiling. In Cambridge one man described one cloud as in great turmoil; "as though two storms had gone together." The man who was so keenly interested in the different air currents described the clouds as moving rapidly; the sky overcast with grey clouds, in which could be seen much darker clouds whose motion could be easily seen. Another man who watched the ship break from a horizontal distance of not more than one and a half miles, and had been watching it for at least 30 minutes prior to that, said the clouds looked somewhat ugly, but not especially threatening; in fact, he said he did not at any time consider it a bad storm, "unless there was one in the upper air." The second fact that seems certain is that during the southeastern movement of the southern low pressure center it was accompanied by some, possibly many, small secondary centers, which one could not hope to plot on a weather map. This condition would naturally be expected in an atmosphere which is very unstable. That such a condition occurred near Ava I am certain, for the barometer readings, taken hourly at the Ohio Power Company's power house at Philo, about twenty miles west of Ava — the only barometer I could learn of within 50 miles — showed steadily falling pressure from 0100 until 0500, after which it began to rise, and continued to do so until 0900, after which it again fell slowly. The power company uses these readings, uncorrected, for elevation, temperature, etc. The actual readings were as follows:

0100.....29:44	0900.....29.41
0200.....29:42	1000.....29:40
0300.....29:40	1100.....29:39
0400.....29:38	1200.....29:38
0500.....29:36	1300.....29:36
0600.....29:38	1400.....29:34
0700.....29:40	1500.....29:32
0800.....29:40	

The pressure of such a secondary would harmonize with the rapidity of cloud and squall formations, the strong southwesterly winds, and the northerly, or northwesterly winds above, and will help to explain the easterly surface winds referred to above, and the northwesterly surface winds at Philo and Marietta, during this period; but it leaves unanswered the question of why the surface winds were not stronger. That other secondaries were present seems certain, from the following barometer reading, obtained from Parkersburg, West Virginia, nearly 50 miles south, where the barometer fell until 0600, and then rose. These readings, like those above, are actual, uncorrected readings:

0100.....29:34	0500.....29:29
0200.....29:32	0600.....29:26
0300.....29:30	0700.....29:30
0400.....29:30	0800.....29:31

My conclusions have already been stated, namely, that the overrunning cold air caused a very unstable atmospheric condition across Ohio, from Lake Erie to West Virginia, bounded on the west, roughly, by a line from Loraine, Ohio, to Elkins, West Virginia, and on the northeast by a line from a short distance east of Cleveland to a little north of Pittsburgh. The instability increased to the point where thunderstorms were formed in the north, a condition which spread rapidly farther to south, where, possibly, the instability was not so great, hence formed detached clouds first, as was noted from the *Shenandoah*, about 0330, in the north and northwest; then these gradually emerged into a solid sheet, as observed from the ship for some time before the time of the disaster, then the thickening of this sheet, in places, into rolls, and later, squall clouds, which appeared about the time the *Shenandoah* was caught in the ascending current. Farther east and north these same conditions finally developed into thunderstorms, while to the south, southwest and west of Ava the process doubtless stopped with a more or less uniform sheet which gave only a little rain. The western limit of the rain appears to have been only about 17 miles west of where the ship fell, 20 to 25 miles southwest, and at least 50 miles south. Accompanying the processes just named were small secondary depressions, which materially aided the

formation of squalls and thunderstorms, through their effect upon the atmospheric circulation in that locality. As this colder air moved east it apparently lost much of its energy, or, in the storm circulations, was dissipated, for the West Pennsylvania Power Company reported, from Charleroy, Pennsylvania, that no electrical storms had troubled their lines on September 3.

The following will describe a little better the drawing together of the *Shenandoah* and the squall conditions which caused the disaster, although it is believed that many more storms occurred than my records show. Shortly after the *Shenandoah* passed over Wheeling, thunderstorms were occurring at Cleveland, Akron, and southwest of Steubenville; the Akron storm being first reported at 0145. The Steubenville storm could doubtless have been seen, and maybe, also, the one at Canton, which was reported as very severe electrically. At 0310 the ship was over the home of Mr. Spencer, six miles west of Washington, at that time there were storms at Canton and Steubenville. At 5 o'clock the ship's position was almost between Cambridge and Byesville; and at 5:20 it was about seven miles southeast of Cambridge. At these times there were storms occurring at Coshocton, Barnesville, and Washington. Of these storms few appear to have been intense, since of eight persons consulted who were within two miles of the spot where the after portion of the ship fell, three saw no lightning, one was doubtful, and four saw it in the north, northeast and east; and three of the latter five heard no thunder. Since this was the case, the storms must have been at a considerable distance, and only a little lightning could have occurred. Those consulted at Byesville saw no lightning and heard no thunder. In Cambridge, roughly 50 per cent consulted saw no lightning and heard no thunder. Farther north were severe storms, however; those along the power company's lines, at Newcomerstown, Canton, and Akron, were reported as severe, and caused violent voltage surges at Philo.

It may seem strange that so many varying reports of the appearance of the clouds near the *Shenandoah* just before the disaster can be obtained, especially to those unaccustomed to constant observation of their forms, yet a little reflection will show that the

same cloud viewed from different angles, or in various light conditions, may give entirely different impressions.

It is evident that the *Shenandoah* was caught in a rapidly rising current of air set up by the squall conditions described, and carried to a great height. During this time it was impossible to keep the ship's heading the same, a condition which would be expected under these conditions, for ascending currents under thunderstorms and squalls generally have a rotating motion. But that the ship was caught in what is generally understood as a "twister," as several times reported in the press, is evidently incorrect, for that implies it was caught in a tornado, and no such storm occurred anywhere near Ava on the morning of September 3, 1925.

Figure 4 shows the weather conditions on the morning of September 3d.

The heavily shaded area is that in which the thunderstorms occur — that is, we have a record at Lorain, at Cleveland, at Akron, and at Canton. The circles represent a radius of five miles, ten, fifteen, 25, 50, 75 and about 100 miles. As the ship crossed near Wheeling, there were storms as I have read in my narrative, and as I have already stated there was a storm at Cleveland, Akron and Steubenville. This storm lasted five hours. At 5 o'clock the ship was in the position shown. There were thunderstorms at Coshocton and Byesville. The lightly shaded area shows where they had clouds and a little rain. The clear area at left shows, roughly, where there were neither thunderstorms nor rain, and not many clouds.

TESTIMONY OF C. P. BURGESS.

Q. State your name and present duty. A. Charles P. Burgess. I am in charge of strength calculations of airship design in the Bureau of Aeronautics.

Q. Will you please state to the Court what experience you have had with rigid airships, particularly those features pertaining to their design and construction. A. I have been in charge of the strength calculations and various other calculations on the *Shenandoah* since the beginning of the design in 1919. That is six years ago. I was in England on the investigation into the loss of the *R-38*, and I made calculations on the *Los Angeles*, and I have car-

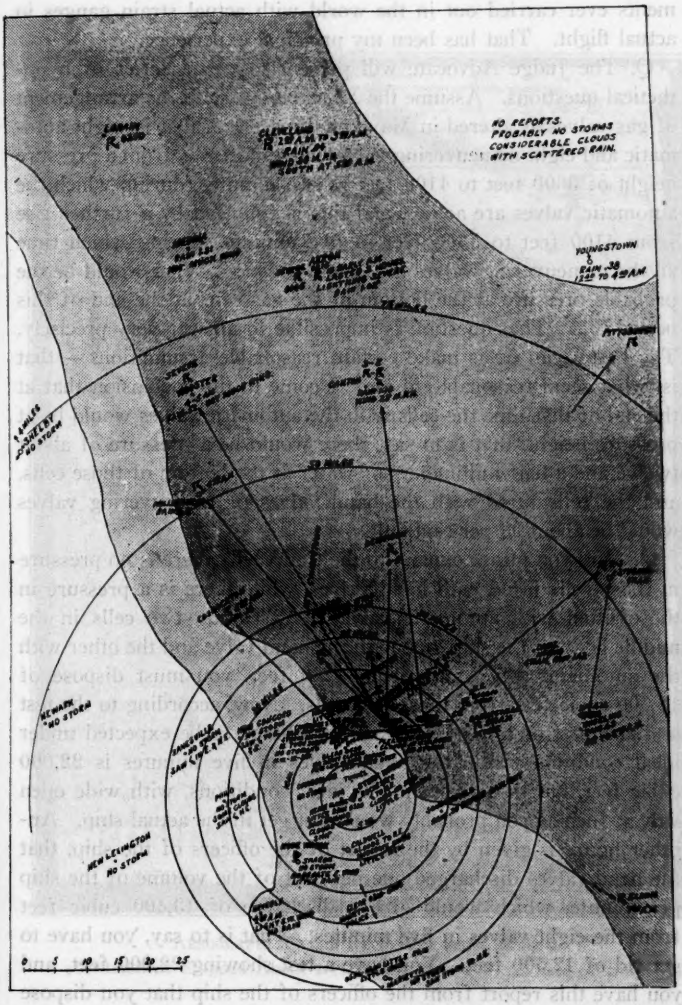
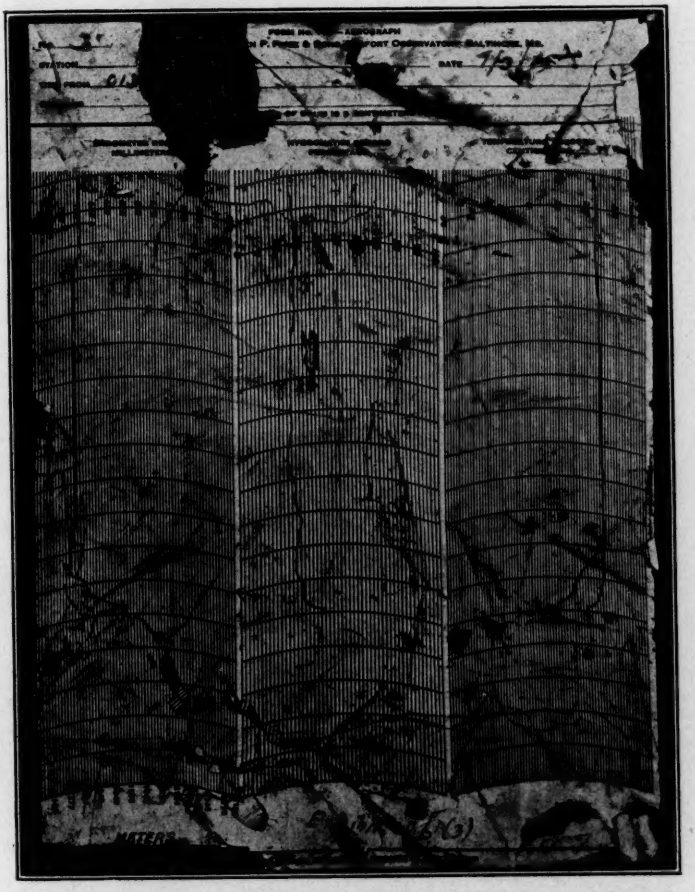


FIGURE 4.—WEATHER MAP OF VICINITY OF WRECK, SEPTEMBER 3, 1925, A.M.

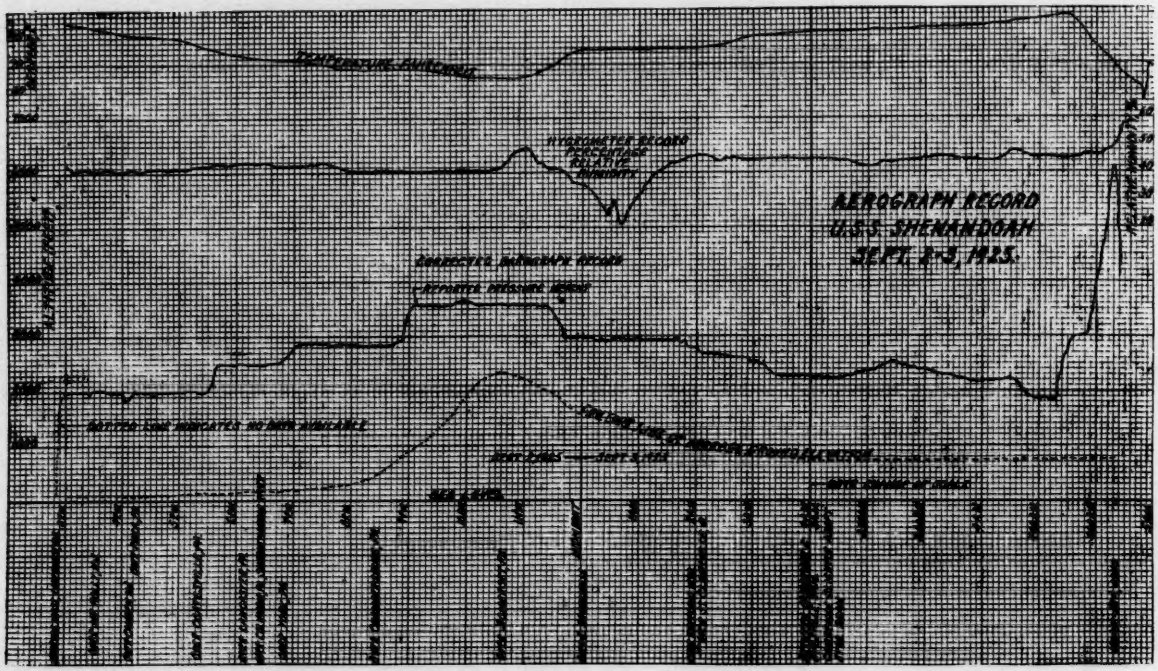
ried out on the *Los Angeles* and the *Shenandoah* the only experiments ever carried out in the world with actual strain gauges in actual flight. That has been my principal experience.

Q. The Judge Advocate will present to you a series of hypothetical questions. Assume the *Shenandoah*, with the arrangement of gas valves as altered in May and June, 1925, that is, eight automatic and eight maneuvering valves; assume a rise from a pressure height of 3600 feet to 4100 feet in two minutes, during which the automatic valves are acting, and this is followed by a further rise from 4100 feet to 5360 feet in five minutes, during which time all the maneuvering valves are open; question, What would be the probable pressure at the bottom of the gas cells at the end of this period? A. That question is impossible to answer very precisely. The best I can do is make certain reasonable assumptions — that is, what seem reasonable to me. I come to the conclusion that at the end of that time the cells with the automatic valves would be at pressure height, that is to say, there would be a pressure of about twelve and a half-millimeters of water at the bottom of those cells, and the cells fitted with the hand valves or maneuvering valves would be about 98 per cent full.

Q. Will you please explain to the Court why there is no pressure in those cells fitted with hand valves, while there is a pressure in those fitted with automatic valves. A. Taking two cells in the middle body of the ship, one with the hand valve and the other with the automatic valve, and rising 1260 feet, you must dispose of 12,500 cubic feet from those two cells. Now, according to the test at Lakehurst of a hand valve, the discharge to be expected under ideal conditions from the hand valves in five minutes is 22,000 cubic feet, but those were under ideal conditions, with wide open valves, such as you probably would not get in the actual ship. Another figure is given by the report of the officers of the ship, that the hand valves discharged one per cent of the volume of the ship per minute, which would give a discharge of 13,400 cubic feet from the eight valves in five minutes. That is to say, you have to get rid of 12,500 feet. You have a test showing 22,000 feet, and you have this report from the officers of the ship that you dispose of 13,400. Now, it is necessary to approximate somewhere between 22,000 and 13,400. Those are the upper and lower limits of



AEROGRAPH RECORD RECOVERED FROM WRECK.
Instrument Stopped About 4.48 A. M., before Final Rise of Ship.



AEROGRAPH RECORD CORRECTED.

the gas discharge. I have assumed 16,000 feet, which is 3500 feet more than the 12,500 called for by the ascent of the ship.

Now, if that excess of 3500 cubic feet were taken equally from the two cells, you would have both cells deflated 1.17 per cent, but it is not probable that the manifold between the cells will give a perfect equalization of the pressure and inflation in those cells, and so I think it is reasonable to assume that you would have the cell without the hand valve full, and letting out a small amount of gas. The other cell would probably be deflated, not 1.17 per cent, but about 2 per cent, leaving it 98 per cent full.

Q. Assume that the *Shenandoah*, with the same arrangement of valves as in the last question, and with pressure 12.5 milllmeters at the bottoms of the cells having automatic valves, and with the cells having maneuvering valves 98 per cent full, makes a rise of seven hundred feet in 42 seconds; and assume, further, that during this rise the maneuvering valves are closed, then what would be the maximum pressure at the bottoms of the gas cells fitted with the automatic valves only, and what would be the pressure in the cells fitted with maneuvering valves only? A. I figure as nearly as possible that at the end of the 700 feet rise you would have in both cells approximately the same pressure, that is, 34.9 millimeters at the valves. Transposing to pounds per square foot, that would correspond to pressures of 6.43 pounds per square foot in the bottom of the cell; 7.15 pounds per square foot at the level of the valve; and 10.21 pounds per square foot at the top of the cell.

Q. Assume the same conditions of rise and altitude as previously specified, and the maneuvering valves to be closed; what would be the pressure at the bottom of the gas cells in the ship with the original valve arrangement, namely 18 automatic valves and 16 maneuvering valves? A. The maneuvering valves being closed, the gas would be discharged from the automatic valves only, and since the rate of ascent is less than 340 feet per minute, there would be no important pressure above the pressure height. In other words, there would be about 12.5 millimeters at the bottoms of the cells during the rise.

Q. My fourth question: Assume a ship under the conditions specified in the previous questions, in which she starts from a pressure at the bottom of the gas cells of 12.5 in one case and 98

per cent filled in the other, and makes a rise of 700 feet in 42 seconds, resulting in a maximum pressure of 34.9 millimeters of water at the bottom of the gas cells; and assume also the valve arrangement to be as modified in the first question, what would be the maximum stresses in the transverse and longitudinal girders, first, due to gas pressure in cells fitted with the automatic valves only, and, second, the gas pressure in cells fitted with maneuvering valves only? A. There will be no important stress in the transverse frames, because I have figured as nearly as possible that there will be equal pressures in both types of cells at the top, that is, 34.9 millimeters, or 10.21 pounds per square foot at the top of the ship. I calculate that that pressure at the top of the ship would cause a maximum stress in the apex channel of the C longitudinals equal to 12,950 pounds per square inch compression.

Q. What would be the stress in the wires in the gas cell netting under the action of the maximum gas cell pressure as given by you in answer to the previous question? A. Four hundred and seventy-five pounds, sir.

Q. What is the calculated breaking strength of these wires? A. Ten hundred and forty pounds tested strength.

Q. Is the pressure, 475 and 1040, given per square inch? A. No, in pounds; total stress in the wire.

Q. Assuming that several of these wires break, under the conditions given by you in answer to the previous question, what would be the approximate effect on the stresses in the girders as given by you in answer to the previous question? A. It is very difficult to tell just what would happen if wires break. Perhaps it is too extreme, but suppose enough wires break to allow the bulge of the gas cell to increase by six inches. I figure that in that case the maximum stress in the apex channel of the C. longitudinal would be about 24,300 pounds per square inch, which is practically the failing stress of that channel.

Q. In stating that the bulge is increased by six inches, do you mean that the total bulge is six inches? A. No; I mean that it is increased by that, that is, the gas cell would bulge practically against the outer cover. That is a very extreme case; I do not think you could possibly exceed that case.

Q. This is question No. 5: Assuming that the ship is flying 1200 pounds heavy, and she rises 1760 feet above the pressure height, the gas cells remaining filled during this rise, what would be the total excess of weight over buoyancy at the end of this rise? A. The excess weight due to the rise is 6170 pounds, and added to the initial 1200 pounds heaviness gives a total heaviness of 7370 pounds.

Q. If the ship rises 2460 feet from pressure height without any discharge of ballast, what would be the total excess of weight? A. 9810 pounds.

Q. My sixth question is this: Assume the ship to be flying 10,000 pounds heavy, what would be the amount and the sign of the bending moment at frame 130, thereby produced due to aerodynamic pressure? A. On the calculation based on Munk's theory of distribution of aerodynamic forces, which is probably fairly accurate, I get a sagging bending moment of 190,000 pounds due to the ship being 10,000 pounds heavy.

Q. What would be the resulting bending moment, if at the same time a hogging moment of 195,000 meter pounds exists at frame 130 due to static loading? A. That leaves a net bending moment of only 5000 meter pounds hogging, which produces a negligible stress of 110 pounds per square inch, which is much less than the inevitable uncertainties in calculation .

Q. My seventh question: What would be the vertical component of the velocity of the air current required to sustain the ship when she is 7000 pounds heavy, while at the same time the ship is going at an angle of 18 degrees down by the head, and at an air speed of 47 miles along the axis, and the ship as a whole ascends at a rate of 220 feet per minute? A. I made it 48 knots. I think you mean 47 knots and not 47 miles. In my calculation I took 48 knots; or 81 feet per second; but there is very little difference in the outcome. I arrived at the upward air velocity to have been 24 miles per hour or 10.7 meters per second.

Q. No. 8: Assuming that a ship descends at a rate of 20 feet per second, what would be the external pressure on the cover, and what would be the maximum stress thereby produced in the structure? It is assumed that the openings in the cover amount to a total of 136 square feet. A. That, sir, is the question hardest to

give a definite answer to. It all depends on the tautness of the outer cover. I think the answer is that in the lower longitudinals, in the base channels, you would have a stress of somewhere between 3500 and 7000 pounds per square inch compression. In the upper longitudinals the stress will be less than if you did not have that condition, because it gives an inward load, opposing the outward gas pressure force.

Q. What do you consider the probable effect of an upward gust under the bow of the ship when she is falling rapidly? A. I have got a long memorandum here which I wrote a week or two ago on that subject, which I will give to the court. I assume this upward gust is distributed upon the fore body with an intensity proportional to the rate of change of cross sectional area. This is in accordance with Munk's theory. I find that if the relative air speed of the gust to the ship (this speed is made up of a combination of the ship's advance and the ship's fall, and the absolute motion of the wind) is 80 feet per second, and if the angle of attack (that is, the angle between the relative motion of the air and the ship's axis) is 45 degrees, you might get a very large upward force of about 30,000 pounds upon the ship, and I assume that that force is opposed only by the inertial resistance of the ship against the angular acceleration and translation. You understand, the upward gust turns and elevates the ship, so you have an inertial resistance to both the angular acceleration and the bodily translation. I assume this inertial mass is distributed along the ship in proportion to the cross sectional area; and I arrive at a maximum bending moment of 600,000 (six hundred thousand) meter pounds sagging, occurring at frame 130. Also note that the change of sign of shear — that is, the minimum shear — occurs at that frame, and this sagging bending moment of 600,000 meter pounds would, according to our strength theory, give you a maximum compressive stress of 13,200 pounds per square inch in the longitudinal at the top of the ship, at frame 130. It is also perhaps of interest and pertinent to the question to say that I figure the angular acceleration due to such force would be three and one-half degrees per second per second as against only .6 degrees per second per second figured as the maximum possible acceleration by maneuvers in still air.

Q. At what point would you expect the maximum bending moment and shear to occur? A. Maximum bending at frame 130; maximum shear at frame 160.

Q. What would be the magnitude of the force at these points necessary to break the ship? A. Well, I think that 13,200 pounds which I mentioned in connection with the gas pressure would put the ship in a critical condition, anyway.

Q. In answering question No. 4 you gave 12,950 pounds per square inch as the stress in the longitudinal girders due to gas pressure. Is that right? A. Yes, sir.

Q. Doesn't that show that the stresses due to gas pressure are as dangerous as those you have given as due to the upward gust? A. No, sir, because the gas pressure stresses I gave would probably not be in conjunction, under the given conditions, with the sagging bending moment, whereas in this latter case you have the sagging bending moment, plus the gas pressure force, which probably produced a stress of the order of 8000 pounds per square inch. So you have, at the time the ship broke, a combination of stress, 13,200 pounds, from the sagging, plus about 8000 pounds gas pressure, giving a total of 21,200 pounds per square inch.

Q. What assumptions do you make to reach that figure of 8000 pounds? A. From the original calculations of the ship. We will assume the ship was not full or was not in pressure, but there was the natural head of gas at the top of the ship due to the lift of gas, which is always there as long as there is gas in the ship, irrespective of super-pressure.

Q. What would be the magnitude of the bending moment and the shear force necessary to break the ship? A. Well, I think in conjunction with gas pressure, the figure I just gave of 13,200 pounds stress, due to the sagging bending moment of 600,000 meter pounds, would suffice to put the ship in a critical condition. I couldn't say just what would happen.

Q. In a critical condition; apt to break? A. Yes.

Q. What shear force would be dangerous? A. Shear force alone? It so happens that at frame 160 you would have a pretty critical shear, at the same time that you have this large bending moment at frame 130 — you would have a shear of 13,100 pounds at frame 160, which would produce in the most stressed shear wire

at frame 160 about 2000 pounds. I think the calculation overestimates the strain on that shear wire, because the joints of the ship and the netting wires all assist to a certain degree to take shear. Therefore the figure of 2000 pounds stress in the most severely loaded shear wire is undoubtedly on the heavy side, although it may have occurred due to excessive initial tension in that one shear wire, or, conversely, slackness in the adjacent wires. So we can take that figure of 2000 pounds as being somewhere near right. Now, the ultimate strength of that shear wire is 2500 pounds, so you haven't got a very large factor of safety there, and it is possible for the ship to have failed in shear at frame 160. But, as a matter of fact, she broke at frame 130. Therefore, the indications are that it was not shear that broke the ship.

Q. In the 13,000-pound stress in compression on C girder, what is the factor of safety? A. I think you mean with gas pressure added. It is about 1.0. Perhaps 1.2 is a better figure to give than 1.0.

Q. Assuming that a car located 10 meters aft from the point at which the ship broke swung down from its horizontal position at the time of breaking, the forward suspensions giving way, to what cause would you attribute this? A. Probably the general break-up of the ship allowed these suspensions to fall away.

Q. Assuming that the control car of the ship tore away and fell very soon after the ship had broken in two, although this car was about 100 feet forward of the place where the break took place, to what cause would you attribute this fact? A. I don't believe that the weight or inertia of the control car could possibly have carried it adrift. The factor of safety in the suspension there is enormous. There are twelve wires, each of which has an ultimate strength in excess of the weight of the car. I believe the car was pulled off by the control wires, which are very much stronger than the weight of the car, as the ship broke.

Q. Do you mean that the pull on the control wires caused the suspension wires to break? A. Yes, sir, or else it may have pulled the car up against the hull, and then the control wires carried away in the after part of the ship and allowed the car to drop, coming up all standing on its suspensions.

Q. Assuming that the snapping of two wires took place shortly before the ship reached a maximum altitude of about 6000 feet; assuming that this break took place somewhere between frames 90 and 140, in the upper part of the ship, what do you consider the probable cause of the break, and would such break, in your opinion, have any influence on the final breakdown of the whole structure? A. Sir, it seems as though that must have been the snapping of gas cell wires. I don't see how, in that part of the ship, anything else could have given. But I don't think that the failure of two wires is in itself anything serious. The worst aspect of such an event is that it indicates that there was high gas pressure. But I don't think that those wires, in themselves, failing, would have caused any serious stress.

Q. Now, we will leave the hypothetical. Suppose the *Shenandoah,* with which you are familiar, were in a condition of 7000 pounds heavy, falling in still air, what would be the rate of fall? A. With no forward motion, the rate of fall, I figure to be about 18 feet per second.

Q. Did you make any examination of the wreck of the *Shenandoah* in or near Caldwell, Ohio? A. I did, sir.

Q. If so, at what time and under what conditions. A. I examined it the following morning. That is, 24 hours after the accident. I really was unable to form any conclusions from that examination. One could imagine all sorts of things, but nothing definite that I could see.

Q. You were unable, then, at that time, to form any theory as to the breaking of the struts supporting the control car? A. I was.

Q. Mr. Burgess, you assumed in reply to a hypothetical question that the discharge of one of the *Shenandoah's* maneuvering valves under 19 millimeters pressure was 16,000 cubic feet in five minutes. I understand that in arriving at this figure you considered the rate of 13,400 cubic feet, estimated by the ship's officers, and the flow obtained by actual test at Lakehurst of 22,000 cubic feet, and took a flow of 16,000 cubic feet. Is that correct? A. Yes, that is correct.

Q. Is it not just as probable that the discharge would be 17,500 cubic feet in five minutes instead of your figure of 16,000 cubic feet? A. It might be, yes, sir.

Q. In that event, following your line of reasoning, at the end of 1260 foot rise, the cell with the maneuvering valve would be 96.7 per cent full after valving for five minutes, instead of 98 per cent full, would it not? A. I agree with your average for the two cells, but I do not think you would have quite that distribution between them. Probably you would have a difference of not more than 2 per cent between the cells, so if the average is 96.7 per cent, probably you would have 95.7 in one and 97.7 in the other.

Q. I read from your answer to a similar question, on a previous occasion. Now, if that excess of 3500 cubic feet were taken equally from the two cells, you would have both cells deflated 1.7 per cent; but now it is not probable that the manifold between the cells will give a perfect equalization of the pressure of the inflation of those cells, so I think it is reasonable to assume that you would have the cell without the hand valve probably full, I mean letting out a small amount of gas, and the other cell would probably be inflated not 1.7, but about 2 per cent, leaving it 98 per cent full. Now, the fact that the maneuvering valve discharges more gas does not influence the rate of flow through the manifold, does it? A. I do not agree; it does make a difference.

Q. In that rise the differential pressure is the same regardless of the amount of gas that passes out of the cell with the maneuvering valve? A. If that is so, let us think. If the cell with the maneuvering valve is only 96.7 full, as you say, where will the bottom be? I might ask you that.

Q. It would be about L girder. A. Now, if one cell was down to the L girder and the other cell full, there would be quite a considerable difference there. Therefore, as I said before, I do not think you would have all that difference in the pressure head between the two cells. I think the difference of 2 per cent in the volume is more reasonable than 3.3; but it does not matter, as I can see it.

Q. Then, in your opinion, the rate of discharge of the maneuvering valves with which one cell is fitted makes no difference in the relative quantity of gas in the two cells. A. I would not say that there was no difference, but under the conditions of the question as stated I do not see how you could actually get a difference of 3.3 in the two cells. I won't agree to your last proposition that there is no difference.

Q. Yet the figure arrived at of 96.7 per cent follows your line of reasoning as given in your testimony on a previous occasion. A. I never, by any line of reasoning, allow such a difference in the inflation of two cells. But I want to add this: that that figure of 12½ millimeters at the bottom of the cell with the automatic valve is wrong. I said that the cell with the automatic valve would probably have that valve just simmering. With that valve just simmering, the pressure at the bottom of the cell is nearer 5 millimeters. I have since calculated further and I doubt if the valve would be even simmering. I think on the former line of reasoning, assuming that the hand valve discharges 16,000 feet, you would probably have the cell with the automatic valve 100 per cent full, and the other cell about 98 per cent full. Now, if you increased the discharge from the hand valve, as Mr. Whittle suggests, you would probably get down to figures of 97.7 and 95.7.

Q. How do you arrive at those figures? A. Two per cent? I made some calculations this morning on the pressure head in the manifold. I had not previously gone into the question of the pressure head in the manifold. I found that a pressure head of only 2.38 millimeters will suffice to push 6250 cubic feet of gas through the manifold in five minutes, assuming that the coefficient of discharge of that manifold is .8. Maybe that is too high, but I allowed .8.

Q. Then you admit that the figures previously given are incorrect? A. What do you mean by that? The figure 12½ is incorrect, yes; but the figure of that cell being full, and the other cell 98 per cent full, is perfectly correct, on the assumption of 16,000 cubic feet. I might think that your assumption of 17,500 is just as far wrong, but that does not make my assumption wrong.

Q. The idea is not to show that your figure of 16,000 was wrong, but we assume, under the same conditions that you did, a difference of flow of 17,500, and following your calculation arrived at a slightly different flow, and you agree that we have a right to assume 17,500 where you assume 16,000. A. Certainly. I might add here, in all the calculations of the strength of a ship I have tried to avoid ever giving the ship the benefit of the doubt. I have always tried to be hard on the ship, as being on the safe side, and in my written answer to the Court, giving the calculations, I said that

16,000 cubic feet discharge is the worse case for the ship, because if the discharge had been any less your maximum stress would have been no worse; if the discharge had been more, your stress would have been less, and therefore I gave a limiting case, the worst case for the ship, and even that limiting case showed no serious stress. I quite admit that you might have less stress; but I do not see how you could have more.

Q. Then your figure of 16,000 was assumed to give an answer? A. No. It happened that I made a guess at that figure, and I found, to my surprise, that it gave the limiting case, the worst case. When you have a range of figures, I think the only thing to do is to take the limiting cases, as I have done.

Q. Then, as far as we have gone this afternoon, our assumption of 17,500 is probably as correct as your 16,000? A. Very likely.

Q. Then if the cell in the maneuvering valve were holding 96.7 per cent after valving for five minutes, instead of 98 per cent, as you stated, then at the end of the five-minute valving period the airship was assumed to rise a distance of 700 feet at the rate of 1000 feet per minute, the change in volume of the cells for a 700-foot rise would not exceed 2.3 per cent; the pressure in the cell with the automatic valve would increase during this 700-foot rise, and gas would escape through the valve and through the inflation manifolds. Considering the area of the inflation manifold, the rate of rise, the automtaic valve characteristics, does it not seem reasonable to assume that in this rise at least one-half of the gas would escape through the automatic valve and the remainder through the inflation thereof? A. No, I don't think so; because the automatic valve is a much bigger area, in the first place, than your inflation manifold, and there is a much bigger pressure head between the cell and outdoors than between the two cells. So I do not think anything like half of the gas would have gone through the manifold.

Q. What is your estimate of the gas that would have gone through the manifold under those conditions? A. I don't know, and it makes no difference in the final answer.

Q. Well, making that assumption which is unfavorable to the ship, at the end of the 700-foot rise, the cell with the maneuvering valve would then be about 100 per cent full, with no super-pressure,

and, as you stated, if the flow were less the cell would be less than 100 per cent full, and the cell with the automatic valve would have a pressure on the valve of perhaps 20 millimeters — do you agree? A. I think it would be more than that. It would not be serious. It certainly would be a good deal less than the 34.9 millimeters that you took into account.

Q. Twenty millimeters corresponds to a discharge through the automatic valves of about 2475 cubic feet, for a rate of rise at the rate of 1000 feet per minute. The gas that would have to be valved, considering one cell only, would be 4950 cubic feet. One-half of 4950 gives 2475 cubic feet. The pressure that corresponds to that is 20 millimeters at the valve. A. It is unreasonable. I do not like to vouch for it; it is unreasonable.

Q. Then in the hypothetical case discussed it is quite possible that the gas pressures obtained with the arrangement of consecutive cells, having maneuvering and automatic valves as actually installed in the ship, would be no greater, and might be less, than in the arrangement where every cell had an automatic valve and maneuvering valves were not operated as in the *Shenandoah's* original installation? A. You say that that is proved?

Q. It is possible. A. It is possible, yes.

Q. Then in summing up, granting that it is as logical to assume the figure of 17,500, which lies between the estimate of the ship's officers of 13,400 and the figures of 22,000, which was obtained by actual test, as is your assumption of 16,000, it is possible under those conditions that this hypothetical ship would not have been subjected to as great pressure with the modified installation as with the original? A. True, true.

Q. The selection of a rate of discharge from maneuvering valves in the hypothetical question submitted to you appears to be of interest, based upon two things: first, upon the rate of discharge reported by officers of the *Shenandoah;* and, second, to some extent upon results obtained in actual tests of valves of this type at the Naval Air Station at Lakehurst. Did you observe any part of the test at Lakehurst? A. Yes, sir.

Q. What is your opinion of this test, as to its ability to represent the theoretical and practical operation of these valves in comparison with any other valve tests, of which you may know? A. I

think it was a pretty good test. The doubtful point in my own mind is whether or not in the actual ship you would get that full 2⅝-inch opening; I don't think you could do it. I am not prepared to say how much you would fall short of it.

Q. But do you know any other tests of valves of this or similar type anywhere at any time that are superior to this test? A. No, I do not know of any having been made that are superior.

Q. In regard to the reported rate of discharge of the *Shenandoah's* valves being approximately one per cent in volume, I offer you a hypothetical question for your consideration and answer. If this reported rate were based on percentage fullnesses read in the gas cells of the *Shenandoah,* either in flight or subsequent to the ship's being placed in the hangar after valving, to land, to what errors would those readings be subjected? And the second part of the same question is this: if we assume that these values have been arrived at based on the loss of lift in valving the ship in preparation for landing, would not the figures resulting give an erroneous low rate of discharge on account of gas remaining in the ship after valving, due to the low speeds and imperfect ventilation of the ship under landing conditions? A. I do not think that any gas discharge from the cells would remain between the cells and the outer cover of the ship more than a very short time. The helium would find its way out very quickly; I do not think there is any question about that. I quite agree to the first part of your question, that there would very likely be considerable error in the estimate of the rate of discharge, based upon computation of the loss of lift. One feature, on the valving of the ship, as done in practice, is, as I believe, that it was found after valving with the hand valves that the cells were deflated rather unequally, showing that the valve openings were rather variable, and as they could never be more than 2⅝ inches, probably some of them were a good deal less. I suggest that to the officers of the ship.

Q. Assuming that there was a variation in the degree of deflation when the maneuvering valves were used in the various cells for the same length of time, what other variable might have influenced this variation in the percentage of gas discharged other than variation in valve openings? A. I don't think the pressure head is very different, because even my figures allow no super-

pressure, and the *Shenandoah* was flown so nearly full that the head of gas, when there was no super-pressure, did not vary very much.

Q. But it did vary? A. It varied somewhat, yes, sir. But remember this: Suppose you are flying along and you have about the same pressure head, with a given amount of gas, no matter whether you are down low, and only, say, 80 per cent full, or higher up, and 95 per cent full, there is some difference, but no very great difference, in the pressure head at the top of the ship under those two conditions.

Q. But is it probable, with a given amount of gas in the cell, that the discharge will be greater at altitude than it will be on the ground? A. Yes, it will be.

Q. Have you ever heard of a case of a rigid airship being valved for the purpose of landing the ship and landing in equilibrium, or perhaps slightly light, remaining on the ground in the hands of the handling crew for only a few minutes, without change in temperature or pressure, and becoming heavy as she lay on the field, due to the loss of gas from between the cells and the outer cover? A. I never heard of it.

Q. In your testimony of yesterday there was some mention of the removal of No. 6 engine from the forward car of the *Shenandoah*. How would this removal influence the ship's speed? A. About 6 per cent, I think.

Q. Assuming the case that was discussed yesterday in connection with the detachment of the control car on the *Shenandoah*, in which you expressed the opinion that it was quite probable that the pull on the control cables had had an important effect in this detachment of the control car, by rotating the control car in a forward direction, what effect, in your opinion, would the operation of an engine in the head direction in this car have had under these circumstances? A. It would have had some effect; but the effect would have been extremely small in comparison with the major forces involved in the pull of the control wires. I think the maximum thrust of an engine is only about a thousand pounds. Perhaps you know more about that than I do. I think it is about a thousand pounds, whereas you had eight control wires, each having the strength of 3700 pounds. So the thousand pounds thrust of

the engine was a small item in the problem. Also you would have the greater weight of the car and the greater weight might under certain conditions have been an advantage, and under other conditions it might have tended to pull it over quicker.

Q. Assuming that the thrust of this engine would have been about 1000 pounds, what was the vertical distance, approximately, between the line of this supposed thrust and the upper point of attachment of the struts? A. I believe it would be about ten feet vertically between the lines.

Q. In other words, approximately 10,000 pounds feet? A. Yes.

Q. What effect would the torque reaction of this engine have had, assuming that the engine at some time or other was subjected to transverse oscillation? A. Very slight.

Q. If the hypothetical questions submitted for your consideration yesterday represent actual circumstances, do you consider the maneuvering of the ship in regard to valves and control of lift and ballast by a ship maneuvered as disclosed solely in the hypothetical questions, — do you consider that that maneuvering was an inferior, ordinary or exceptionally fine performance, in meeting the conditions set forth by the hypothetical questions? A. The maneuvering of the ship as actually done?

Q. As represented by the hypothetical questions, solely that way. A. Did the hypothetical questions concern the maneuvering of the ship?

Q. The maneuvering of the ship in regard to valves and control of lift and ballasting, by a ship represented as being maneuvered as appears in the manner set forth in the hypothetical questions. A. No, I am sorry to say that I think the event showed the disadvantage of that type of valve system; because by the hypothetical questions the ship is supposed to have risen 700 feet at the rate of 1000 feet a minute with the hand valves closed, which I think was bad.

Q. But do you consider that the relative accuracy with which the final volume of gas was reduced represented at least a reasonably accurate judgment of the situation? A. No; I think it was purely guess work. By the hypothetical question you stopped valving at a certain point. After stopping the hand valves the ship rose 700 feet at a thousand feet a minute, and by the grace of God she stopped rising at that point.

Q. How heavy was the ship at the top of the rise? A. We figured it out the other day. I think it was 7000 pounds. At the very top, the highest point of all, she was 9810 pounds heavy, on the assumption of no ballast discharged.

Q. Then the ship was, by the operation of her valve system, rendered approximately 10,000 pounds heavy at the highest point? A. Yes.

Q. Did this 10,000 pounds heaviness have a considerable deterring effect on further rise? A. You would think it would; but at the same time that 10,000 pounds did not come all of a sudden, and we know the ship rose very fast and suddenly. Therefore, that sudden change from going up so fast to down so fast must have been mainly the effect of change in air currents.

Q. Had the heaviness of the hypothetical ship at the top of the hypothetical rise, had that heaviness been made any greater than as indicated in the questions asked yesterday, what would have been the effect on this ship during its ensuing rapid descent? A. It would come down faster, I should imagine.

Q. To prevent its ultimate crash on the ground would have required what? A. The dropping of more weight.

Q. And had the ship been made heavier than as established in the hypothetical question, and assuming that the forces of nature cannot be controlled for the purpose of solving hypothetical problems, and that the same forces existed in the second condition, where the ship is heavier, and fell no more rapidly than existed in your solution of the questions submitted to you yesterday, what would the effect upon the structure have been had the weight been heavier? A. You mean if they had valved more they would have been in a worse position — yet I claim they should have valved more. As it was, they did valve exactly the amount apparently to get the ship just right at the top of the peak. Therefore, they did just the right thing; and yet I cannot say that it was in accord with good practice; it was mere chance that you happened to get just that 700-foot rise; it came out to be just right with the amount that they had valved; but at the same time I do not admit that the method of letting your gas out of your cell so you are only 98 per cent full at one point and then that you have a high superpressure at another point, is the best way to do it; it is not the best way to do it.

Q. My last series of questions were not aimed at the establishment of the suitability or the particular desirability of the gas valve system; they were rather aimed at a substantiation of the faith of the Bureau of Aeronautics and various other organizations in the ability of the personnel of the ship to operate, with the particular system installed, and I am satisfied from your answers, since there was no one else present to operate the system, since it did result in precisely the correct situation that the combination of this valve system and the personnel of the ship was a satisfactory one. Do you consider that the results, based on your hypothetical questions and hypothetical solutions bear out that fact? A. No, because the point is that up to this point you had, say, *Shenandoah* luck, and after that it deserted you; but I think the whole hypothetical story proves that that system of valving is not as good as systems of proper automatic valve capacity in each cell.

Q. You stated that you did not consider that all of the maneuvering valves were open an equal amount due to the fact that the percentages of valves of the various cells were not the same in the valve? A. Yes.

Q. Now, consider the installation in the ship, all maneuvering valves were of equal size, all cells were not of equal size, and therefore the percentage lost of gas for a given period of openings of the valves would change with the various cells, depending upon the size of those cells? A. Yes.

Q. Therefore the fact that the percentages of valves at the end of the valving period were not all the same would not necessarily indicate that the valves were not open, but might indicate that the valves were all open and in equal amounts. A. True, if they varied in that way, yes.

Q. Then if your assumptions, which you state represent the worst conditions, are concurred in, your testimony will show, then, that the ship was never subjected to such a super-pressure as to necessarily injure the structure. Is that correct? A. Yes, correct.

Q. And if the figure of 17,500 is correct, there was even less super-pressure on the structure? A. Yes.

Q. Is the Court to understand that in the hypothetical questions given you, the pressure on the structure of the ship, with the arrangement of valves as installed on the *Shenandoah*, was possibly

no greater than would have been the case with the original installation under the same conditions of rise if maneuvering valves were used in the former case and the relief valves had been relied on only in the latter case? A. Possibly the stress was no greater.

Q. When the question of the changing of the valve system of the *Shenandoah* was under consideration, was it referred to you? A. Yes, sir.

Q. Did you then make any calculations as to the effect of this change in various rates of rise? A. Yes, sir.

Q. Do you remember what your findings were? A. I wrote the final letter from the Bureau authorizing the change, and I stated that the safe rate of rise, without the use of the hand valves, was considered to be 400 feet per minute, and that with anything in excess of that the hand valves should be used.

Q. And then did you concur in this change on the representations made to you? A. I did not like it, no. I thought it was a mistake to change from an automatic feature to a manual feature; but I admit, granting that the crew looked out for that feature, it would not jeopardize the ship.

Q. Then from your answer as given just now, does the court correctly infer that the crew looked out for it in that particular respect in the handling of the maneuvering valves? A. No, sir. I claim that they did not. They allowed the ship to rise at a thousand feet per minute without those valves; but at the same time I also figured out that even that severe condition did not introduce by itself a strain sufficient to jeopardize the ship.

Q. I understood you to say, if I remember correctly, that in that rapid rise, at the peak of that rapid rise, the condition of the ship was just as she should have been, taking into account weight and so on? A. In her weight, yes.

Q. And also you said that it was just right then, but by the grace of God she did not go higher; is that right? A. Yes, sir.

Q. At that point, then, she was correctly handled? A. No, I think I would put it this way. The point is they got the correct result, but they did not get it by the right method. The right method would have been to have kept the ship just full all the way up, and instead of that they got her partly deflated, and then came the sudden rise which just happened to fill her up at the top of that rise.

Q. Then, as I understand you, you attribute that to accident rather than to correct manipulation? A. Yes, sir.

Q. Would it not have been possible by training or otherwise to have made the accomplishment that you speak of absolute? A. Sir, I do not see how it would be possible, how the people in the control car could handle the maneuvering valves to make the ship just full all the way up, it is not possible; that is why I consider the automatic valve to be superior to a hand operated valve.

Q. Then you do not in any wise criticise the operation as it was done? A. No, sir.

Q. In answer to the question as to what you considered the probable effect of an upward stress on the bow of the ship, when she is valving rapidly, you stated that the relative air speed of the gust and the ship on this speed is made up of the combination of the ship's advance and the ship's fall to the absolute motion of the wind. Suppose you had a relative velocity of 80 feet per second. In the elements making up that 80 feet per second, what did you assume as the absolute motion of the wind? A. I did not try to resolve it into its elements; I merely found that velocity of 80 feet per second, in conjunction with gas pressure, would put the ship practically at the breaking point, and I did not try to find out how that 80 feet per second might have occurred.

Q. Could you break it down approximately into the elements you have named? A. I do not see how I could sir; it would be guesswork.

Q. Is that, however, without an attempt to break that down, certain to be a figure within possible limits? A. Yes, sir; it seems reasonable.

Q. You say it would be reasonable. What approximate value would you assign to the absolute upward motion of the air? A. We did figure out in a previous case 24 feet per second, which, with the fall of the ship, gives about 35 feet per second relative upward velocity, and then apparently the ship got out of that upward current, and then she might have gotten into it again when this final crash came.

Q. Would the other elements, with reasonable values, make up the difference between the 35 feet per second and the 80 feet per second? A. I think so. There might easily have been a horizontal component to make up the balance.

Q. What sort of a horizontal element in magnitude would you have to have had? Add 25 feet to that in a vertical direction, would that result in reasonable assumptions of speed and angle of ship? A. I think so, sir; but perhaps the 80 feet was not in a vertical plane; it may have been inclined, you see. It was not necessarily vertical, but across the axis in some way.

Q. The components would have to be corresponding, to figure the upward air and the dropping assumed in a vertical direction. A. You have a triangle there with the hypotenuse 80 and the vertical limb 55. That would be about 58 feet horizontal, would it not?

Q. And that would be a speed of about 50 miles an hour? A. No; that would be 40 miles per hour.

Q. At 58 feet per second? A. Yes, sir.

Q. Then, roughly, you would reach the condition which you give in answer to the question which I first quoted, that it might reach that condition with an upward air current, roughly, in the neighborhood of 12 meters per second? A. Yes, sir; or even ten I think would do it.

Q. Have you kept in close touch with the development abroad, or elsewhere, of rigid airships? A. Yes, sir.

Q. In the further development of rigid airships, since the design of the *Shenandoah,* have any characteristics been discovered that differ fundamentally from the *Shenandoah* in strength elements? A. Fundamentally is a big word. I would not say fundamentally, but there have been quite a lot of differences. In fact, it may be of interest to say here that the *Shenandoah* was not designed by our own figures. I made straight calculations, but they were merely to confirm sizes that had been determined upon by closely copying a German prototype, and as we worked on the calculations there were many features of the ship that seemed to us not right, but we dared not put our inexperience against German experience, and we stuck to our model. Later, in the *Los Angeles,* we were gratified to see changes in many cases along the lines that had seemed good to us as the result of our study of the *Shenandoah.*

Q. At that time, then, are we to understand that you did not consider the design of the *Shenandoah* fundamentally sound? A. Oh, no, I do not say that; but I say that I do not think the

material was disposed to the best possible advantage. The secondary longitudinals weighed practically two-thirds as much as the mains, and helped very little in the strength of the ship. I do not think she was weak, but I say that I do not think there was the greatest economy of material in the *Shenandoah.*

Q. Well, my particular questions are directed at the strength of the ship to resist any aerodynamical strain that might be brought on her, when I speak of fundamentals, that is what I mean. With that in view, what would your answer to my first question be? A. Yes, I would say a fundamental change would be to make the ship very much shorter and fatter; that would make the ship very much stronger against aerodynamic forces.

Q. As far as her strength to resist these forces was concerned, was the *Shenandoah* in her design sound as far as the art had gone at the time of her building? A. Yes, I think I may say that the *Shenandoah* fulfills all strength criteria which we have ever come across in our experience or what we have learned from abroad. I would like specially to point out the case of a Parseval airship, *PL-27,* which was a non-rigid airship of somewhat over a million cubic feet, very much the same shape as a Zeppelin, but a non-rigid airship. Her form was maintained by internal pressure, and her designer states that in the course of two years' service she mastered squalls with an operating pressure of only about 20 to 25 millimeters, and, figuring backwards, you can arrive, as he arrives, at an empirical formula for the maximum bending moments in airships. I have applied that to the *Shenandoah,* and it comes out all right. We also tested that formula by the strain gauges in the flight of the *Shenandoah* across the Alleghenies in the first month of her service, and the stresses were about 10 per cent less than by that formula. I think it is particularly interesting to note that the *PL-27* served in the Baltic in 1915 and 1917, where there is to be found as bad weather as anywhere in Europe; and yet there was no trouble.

Q. Then you do consider that the *Shenandoah* was as strongly built as it was possible to build an airship of that class and characteristics? A. Why, no, sir, not as strong as it was possible to build, but I think it was as strong as seemed required from our knowledge of aerodynamic forces.

Q. Have you carried out any field experiments to determine the stresses of the *Shenandoah?* If so, how do the results check up with your theory of aerodynamic forces? A. I just explained to the Admiral that in the flight to St. Louis in October, 1923, crossing the Allegheny Mountains, we got pretty severe pitching and yawing; in fact, full 30 degrees rudder and elevator were required to hold the course, and the maximum stresses were about 10 per cent less than one would expect according to that empirical formula I spoke of. I may also add that we have data on the German method of strain calculations, and the *Shenandoah* comes within that, too. She is amply strong by either method of calculation. There is not in airship design any standard gust compared to the standard wave in naval architecture. We have had several different theories during the construction of the ship. One was that the worst strain was due to the tail surfaces waggling the ship, the rudders and elevators applying the transverse force. Another case is flying at a fixed angle of pitch. We applied all those theories to the *Shenandoah* and she seemed to be right within reason, although it is perfectly true that if you take an angle of pitch or yaw of 45 degrees at a good speed she would certainly break. There seemed no reason to believe that such angles were liable to occur in service, although we always realized that in tornadoes or even in thunder squalls, it might occur. The theory was that we would have to avoid such squalls, and we thought they could be seen.

Q. Suppose we return once more to the hypothetical question and the hypothetical ship. Suppose this hypothetical ship had been so maneuvered and the gas so valved that the cells were just full at what we will call blowing pressure throughout the rise; would she have arrived at the same terrestrial elevation, the same elevation above the earth — I am using that phraseology to avoid using the phrase "the top of the rise" — as the result of having greater or less upward forces acting on it? A. Yes, I agree that as it was, if she had had the original system she would probably have risen higher, because she would have been full all the time.

Q. Or at a given elevation above the earth's surface she would have had a higher upward velocity? A. Yes, sir.

Q. And to prevent further rise due to that higher velocity would have necessitated the valving of still greater volumes of gas, would it not? A. Yes.

Q. And the ship finally would have been at a greater altitude? A. Yes.

Q. And the ship at that final position would have been heavier by a still greater amount? A. Yes.

Q. And in her ensuing descent she would have descended more rapidly? A. Yes.

Q. And she would have been confronted with the necessity for valving still more? A. Yes, it works out that way.

Q. And in the subsequent and final upward climb on account of the higher velocity with relatively little diminution in the inertia of the ship due to greater weight, the stresses too upon the ship's structure would have been even greater, would they not? A. Yes, that is true, but if you mean to make me say that I think that is the best system of handling, I do not agree.

Q. Is it not true that your practical airship operating experience is of decided less extent than your theoretical experience? A. Yes, that is true.

Q. (By Professor Hovgaard.) Mr. Burgess, in your previous testimony you submitted a memorandum on the loss of the *Shenandoah*, in which you gave a calculation of the stresses in the longitudinal girders of the ship under certain assumed aerodynamic conditions. A gust of eight feet per second velocity at an angle of attack of forty-five degrees was shown to produce a maximum stress in the top longitudinal of 13,200 pounds per square inch. In your previous testimony before the Court you stated that with the gas pressure added this would be sufficient to put the ship in a critical condition. Please explain this statement, showing that the total stress would approach the breaking stress of the girders under the conditions of the final rise of the *Shenandoah*. State where in the girders this stress would occur. A. Besides the primary bending moment producing that 13,200 pounds per square inch, there is the gas pressure force. We can assume that the ship had dropped 3000 feet from the highest altitude, so that she was about 3000 feet from the highest altitude, about 90 per cent full of gas, lifting .0545 pounds per cubic foot, having been at a height previously where the gas had a unit lift of .049 pounds per cubic foot. As the ship drops and the gas is compressed, it increases in unit lift because there is no change in total lift. I have made a calculation of the gas pres-

sure at the top of the ship when she is 90 per cent full of gas, having that lift. In that case you would also have the rise of the intermediate frame, and from an actual gas cell test carried out at Lakehurst, I found that the lift of the intermediate frame gave a stress of about 4090 pounds per square inch. The calculations show a total stress of about 20,480 pounds per square inch, made up of the 13,200 from primary bending, and the 4090 from the lift of the intermediate frame, together with 3190 from C longitudinal. And also, in this case, we will not assume that the outer cover is helping you. Before, I assumed there was a tension in the outer cover that would partly relieve that stress in the top longitudinal, but we want to try to get as hard a case for the ship as we can. In all the stress calculations I have tried to be hard on the ship. So in this case we will leave out the outer cover, and we get a total stress in the top longitudinal, either the A or the C longitudinal, of about 20,500 pounds per square inch in the apex of the channel at the joint. The strain gauge experiments show that, due probably to irregularities in the tensions of the wires, the stresses might easily vary at one spot 25 per cent either way, plus or minus, from calculation. So if we add 25 per cent on to our 20,500, we have almost 26,000 pounds per square inch. That is practically the failing stress of the girder. The girder has a failing stress of from 26,000 to 27,000 pounds per square inch. But perhaps the girder will be more likely to fail just outside of the joint, for two feet either side of the joint, the lattice is double, giving the girder a strength of from 26,000 to 27,000 pounds per square inch. Outside of that double latticing, that is, at two feet from the joint or further, the latticing is only single, and the failing load of the girder is reduced to about 20,000 pounds per square inch. Of course, the stress in the girder is also less, because as you go further from the joint you get a smaller bending moment, and I figure that at two feet from the joint in this condition you would have a stress of about 17,000 pounds per square inch, and if we then add 25 per cent for good measure, you get to almost 22,000 pounds per square inch, which is more than the crippling stress of the girders in the single lattice portion, so that you might have failure either at the joint, or more likely just outside the double latticing.

Q. Mr. Burgess, I ask you whether in the test of the gas cell which you speak of the conditions were not so different from what they would be in a complete ship that your inference of the stress of 4090 pounds per square inch due to the rise of the intermediate frame would not be invalidated in that way. A. Well, in a certain sense, of course, the outer cover would assist. In the ship the influence of the outer cover would tend to reduce the stress, but, on the other hand, at the age of the ship you can imagine that some of the shear wires would probably have stretched due to yielding of the terminals or other causes. They do undoubtedly stretch with time, so that more motion of an intermediate frame is possible. Again, that figure of 4000 probably errs on the side of attributing too much stress rather than too little.

Q. When you speak of the margin of 25 per cent, the allowance you made over and above your calculated stresses, on what do you base that estimate of 25 per cent allowance or margin? A. Mainly from strain gauge experiments. I have no strain gauge records from the *Shenandoah*, because on the *Shenandoah* we merely had the indicating apparatus. I just had to see things and note them down. On the *Los Angeles* we had a recording apparatus. It seems that certain girders will receive more than the figured stress, and when you consider how complicated the structure is and how the wire terminals are made, I do not think that it is at all surprising that there should be those irregularities. Even in the celluloid model tested at the Massachusetts Institute of Technology in Cambridge, we found the same sort of thing; certain members stressed beyond the theory and others less. And the departures from theory seemed to be erratic.

Q. What aerodynamic forces would have sufficed to break the *Shenandoah* when at the maximum altitude of about 6000 feet, when presumably the gas pressure forces were greatest, and when, according to your previous testimony, a comparable stress of 12,950 pounds per square inch existed in the C longitudinal? A. I figure that a bending moment of 190,000 meter pounds sagging, plus that high gas pressure, would have loaded up the girders to about their crippling stress. That is, a bending moment of 190,000 meter pounds could have been produced by a gust of about 45 feet per second, distributed in the same way as the gust of 80

feet per second which I previously gave for the final condition when you did not have the high gas pressure.

Q. Do you think it probable that such forces occurred? Such a gust occurred, or that forces sufficient to seriously strain the ship did exist at that time, due to this cause or other causes? A. It does not seem likely that at the top of the peak she would have experienced such a gust, because probably the ship would have been turned at a very sharp angle up by the bow, and so far as I know she did not have that experience until the final break.

Q. You do not think, then, that the existence of such a gust would be so likely or was possible, but do you think there were other causes, such as higher gas pressures in the cells, which might have caused excessive strains in the girders? A. No, sir, I think that the figure that I gave before for gas pressure seems the highest possible. In fact, since I was last here in the Court I have more accurate figures on the discharge capacity of the manifold, and it seems that the figures I gave you before as what I considered the outside maximum were probably a little too far outside, and I think possibly the stress at the top was nearer 11,000 rather than 13,000 pounds per square inch. As I said at the time, I was trying to obtain an outside figure.

Q. The gas pressure at which you arrived in your previous calculation was, I believe, 10.2 pounds per square foot. Is that correct? A. At the top of the ship, yes, sir.

Q. Now you think that probably the pressure was actually smaller than that? A. Probably nearer eight pounds per square foot.

Q. Then, in fact, you do not see any possible explanation why the ship should be overstrained when she was at the maximum altitude? A. No, sir.

Q. We only know that there was some snapping of wires when she approached the peak of altitude. A. I cannot see how excessive pressure would obtain at the maximum altitude, unless some maneuvering valve failed to function. That is the only explanation I can see likely.

Q. How else can you explain that some of the wires snapped? A. Some of the wires may have been at greater tension than their neighbors, and therefore took an undue portion of the load; or

maybe the snapping would not necessarily mean a parting of the wires. The wires often make noises without actually parting.

Q. In what way do you mean — by adjustments? A. By adjustments or perhaps a slipping of the terminal would cause a noise.

Q. Do you think it most likely that if wires snapped or did adjust themselves, that it was shear wires or gas netting wires that behaved in that way? A. Well, it is pretty hard to speak definitely on such a question, but I think the chances were rather in favor of its being a gas cell wire. I feel that is an opinion only, without much weight to it.

Q. In the assumptions which you made in calculating the stresses due to gas pressure when the ship was at the peak of altitude, did you make an allowance for stretching or yielding of the attachments of the gas netting wires to the structure? A. No, sir; I did not.

Q. If you had made such allowance, on what would you base such an allowance, and what would be the result? A. I have made an extreme allowance by assuming that the modulus of elasticity E of the wires was reduced from 30 million to 15 million pounds per square inch. That is a big reduction. I know other designers have also made similar deductions, but rarely as great as that. Taking that extreme deduction, and also taking the extreme gas pressure of 10.2 pounds per square foot at the top of the ship, I found that the load on the longitudinal would be increased about 1350 pounds per square inch stress, which is not very great, particularly as I think that that gas pressure of 10.2 per square foot is too large. I think 8 pounds is nearer the truth.

Q. Would that be in the C longitudinal? A. In the C longitudinal, yes.

Q. Speaking of the time when the ship finally broke, what, in your opinion, is most likely, that the ship broke essentially due to aerodynamic force of the gust combined with gas pressure which had existed at the time without any previous weakening of the structure, or, the second alternative, that the ship was already weakened by excessive stresses when at the peak of the altitude, and in the latter case what injury would you consider most probable to have happened when at the peak of the altitude? A. I do

not see how gas pressure could have been at all serious at any time, unless, as I said, some maneuvering valve had failed to operate properly. Therefore, I think the ship was undoubtedly broken by a great aerodynamic force after she fell from the maximum height down to the 3000-foot level, and we know that at that time she was suddenly turned up at a very steep angle of inclination, showing a very great air force upon her.

Q. You do not consider it likely that any weakening existing in the ship at the peak of the altitude would have any great influence? A. The pressures do not seem great enough to have produced any such weakening.

Q. The Court would like to interject there just for a moment to understand what he says. Does that hold with the highest point reached by the ship, as far as any testimony you know of is concerned? A. Yes.

Q. If, then, we assume that the ship broke entirely or essentially by the aerodynamic force, and that the ship was not previously weakened, do you think that the ship broke in hogging or sagging? A. I think, sir, the crippling of the upper girders must have occurred in sagging. That does not necessarily mean that they actually parted under that condition. The ship turned up to a steep angle, and then apparently stopped turning up, showing an applied force to turn it down against its upward rise. That applied force to turn the ship down again, producing hogging, would have been practically the inverse of the initial force that turned it up in the sagging. In order to account for the ship actually pulling apart in two parts, we must assume that somewhere there occurred a very heavy tension and the strength of the undamaged girders in tension is so very great that it would seem as though the extreme tension must have occurred after the girders had been previously broken or crippled in compression. Therefore, the action of the gust may have been to turn the ship up with a heavy sagging bending. At that time some of the upper girders were crippled. Then, as she ran out of the gust and the force was reversed, she possibly had a hogging moment; that is, the girders which had been crippled in compression then received a tensile force which would very likely break them. Of course, I cannot give any figures on the tensile strength of a girder already crippled in compression.

Q. Changing the subject, what is in your opinion the range and magnitude of vibratory stresses in the *Shenandoah* due to the engines or propellers? A. The strain gauges used have a period of about 300 cycles per second; that is, they can register vibrations of 300 cycles per second, which is, of course, far more than the engine vibrations. These records are not from the *Shenandoah,* but from the *Los Angeles,* which has the same character of structure. So that the fact that these records show distinct lines rather than broad bands is proof that in the *Los Angeles* at least there were no heavy stresses from engine vibrations, and I think the same would undoubtedly be true of the *Shenandoah,* and certainly one never felt on the *Shenandoah* any serious vibrations. I have lain down for a long time at the hatch from the hull down into the engine car, right over the strut connections, and never felt any heavy vibration there. So that I think the fact that no heavy engine vibration could be felt, coupled with these records, is proof, or at least extremely strong evidence, that there was no serious stress from engine vibration in the hull proper.

Q. Have you noticed whether there were any vibrations set up by the propellers of the ship? A. I could not separate engines from propellers in that respect, and the period would not be so very different, so the same argument holds.

Q. A previous witness has pointed out the importance of having diagonal wiring in the longitudinal plane connecting the A girder with the keel. He stated that such wires insured the effective operation of the hand valves by preventing deformation of the hull. Will you please explain to the court why these diagonal wires were omitted in the *Shenandoah,* and state your opinion as to their usefulness? A. The *Shenandoah* was copied in the main from the German *L-49,* which never had such internal wires. They used the internal wires in the *L-30* class built the previous year to the *L-49,* and not so much refined. Later on they returned again to the internal diagonal wires in the ships that had 15-meter spacing of the main frames, using the internal wires to support the corridor between the main frames. In the *Los Angeles* they have a very strong keel corridor and again have omitted the internal wires. It is obvious that in any case the internal wires could have been very slightly effective in preventing a vertical

elongation of the main frames, because in these frames you had very strong wires right in the plane of the frames, very much more effective than any inclined wires between frames.

Q. What in your opinion was the effect of tying off the manifold between cells Nos. 8 and 9 at the time of the rise of the *Shenandoah*, what we call the first rise, about fifteen minutes before the breaking of the ship? Would that fact, that the manifold was tied off, have any influence on the strength of the ship or any influence on the loss of the ship if such tie-off took place? A. Cells 8 and 9 both had automatic valves, so that tying off there would have done no harm. In fact, I think it would rather help out, because it would probably somewhat reduce the gas pressure in the after cells. I think it was the proper thing to do.

Q. Mr. Burgess, you stated that when the *Shenandoah* was at the peak of her rise, if prior to that time one of the maneuvering valve controls had broken, it was quite likely that gas pressure would have been built up sufficiently high to cause failure of the gas cell structure? A. Yes, sir.

Q. How probable do you consider such a failure of the control wire to occur? A. It does not seem probable at all. Strange things happen. It is very hard to give a definite answer to such a question, "how probable."

Q. Considering the fact that the maneuvering valves had a control lever, so proportioned that for each inch that the valve lifted the control wire moved 3.45 inches, and considering the fact that the valve when fully opened would be open 2.6 inches, to open the valve fully the control wire would have to move the product of those two figures which I gave you, or nine inches; and in the control car there was a reel on which these wires were wound up nine inches; isn't it probable that breakage of a wire in that operation would have manifested itself to the officer operating the controls? A. It is certainly probable.

Q. And if it did not manifest itself to him at that time, would it not have manifested itself as soon as the controls were released? A. It ought to have.

Q. Had you ever heard of the *Shenandoah* having a maneuvering valve failure or a maneuvering valve control failure? A. No.

Q. Never during her entire history? A. No, sir.

Q. Were you on the break away flight of the *Shenandoah* on the last of January, 1924? A. Yes.

Q. You took strain gauge readings preceding that flight? A. I did, sir.

Q. And during that flight? A. Not during the flight, no.

Q. Did your strain gauge observations taken before the flight and immediately before the breaking away, show any excessive readings? A. No. They showed stresses of about 8600 pounds per square inch. When the ship broke they all dropped to zero, practically. Just before that they had been running 8600 pounds per square inch.

Q. Was that value which you have just given sufficient to cause any permanent deformation? A. No.

Q. In today's examination the question was put to you in regard to influence on loads on structural members of the ship's structure, that yielding of gas cell wire terminals and fittings might have. Presumably in response to that question you discussed your assumption of the modulus of elasticity of wires of fifteen million as against thirty million. A. Yes.

Q. Will you please explain to the court exactly what the effect of this assumption is? A. The effect of reduced modulus is to increase the stretch of the wire under a given load; that is, netting wires would bulge more, with the result that the actual tension in the wires themselves would be less, but they would throw a greater load on the longitudinals because of the more unfavorable angle.

Q. So that, from that point of view, if we assume a set of conditions that unduly or duly perhaps adds to the load of the structure, we must, in order to do so, make assumptions that are inconsistent with the probability of failure of the wires concerned. Is that correct? A. Well, perhaps I did not make it quite clear what I meant by reduced modulus. Of course, the modulus of the actual wires cannot be reduced, but the effective modulus, including the wire terminals, can be reduced.

Q. But isn't it correct, as I have asked you here, that if the over-all modulus is reduced to fifteen million, the actual modulus of the wire itself necessarily remaining at whatever it was, thirty million, you then conclude that due to the assumed lower modulus a greater stress may be brought upon the structure, due to greater

deflection of the wire, but that the actual load in the wire itself adhering to this lower modulus, must necessarily be less. Then am I correct in concluding that if we will assume a set of conditions of this sort tending to increase the loads on the structure, we are at the same time compelled to assume a particular condition in regard to the wire itself which would lead us still more to doubt that any of those particular wires failed? A. Yes, that is so. The wires that bulge most would be least liable to fail.

Q. So that the assumptions that lead us to conclude that there may have been higher loads on the structure of the keel will lead us to conclude that the wires had less probability of failing. A. That is true.

Q. Do I understand that your calculations on assumptions based on the evidence that has been put before you convinces you that there was or was not sufficient internal gas pressure at the highest point of the rise to burst a cell or to seriously weaken the A or C girders? A. Yes, sir. If the hand valves did not fail in any way, and we have every reason for believing that they did not fail, the calculations show that there could have been no excessive gas pressure in the ship at any time.

Q. Even at the highest point of the rise? A. Even at the highest point of the rise; yes, sir.

Q. There has been some testimony and much talk of the *Shenandoah* being heavy in her hull structure. Do you know if it is true that she was heavy or not heavy, on what basis has such a report been circulated, do you know? A. Yes, sir; I think I can answer all of those questions. The weight of the hull of the *Shenandoah*, including the gas cells and the outer cover and the valves and all things of that sort, was about 41 per cent of the gross lift with helium, whereas in the *Los Angeles*, the very latest German airship, the figure is about 37 per cent as against 41 in the *Shenandoah*. That difference is due in a large measure to the fact that in the *Shenandoah* there was a greater subdivision of the gas space. There were more main frames, only two-thirds as far apart as in the *Los Angeles*, which somewhat increases the weight, but at the same time gives you more safety, in the same way that increasing the number of water-tight bulkheads increases the safety of a steamship.

You also spoke, Admiral, of the comparison with other German airships; I mean, you spoke of the origin of the report. It is true that the *Shenandoah* was a good deal heavier than certain wartime German airships, built especially for high altitude bombing, in which it was necessary above all things to have extremely light ships. They cut down strength, power plants, and everything they could, to attain high altitudes for bombing London and other places. The *Shenandoah* is heavy in comparison with those ships.

Q. Then is it or is it not a fact that such heavy construction of her hull structure added to or subtracted from her strength as a rigid airship? A. It added to her strength; yes, sir.

Q. Then any criticism of the *Shenandoah* as to the fact of her being heavy in her hull structure is unwarranted? A. Yes, sir.

Q. Can you tell me in regard to her structural weight, whether any comparison was made between the actual weighed on weight and the estimated weight of the design? A. Yes, sir. The actual weighed on weight was about 3.64 per cent more than the estimated weight, which I think is very close for the first American rigid airship.

Q. Returning to your statement that at the highest point reached by the *Shenandoah* on the assumptions given you, you do not believe the stresses due to gas pressure could have been of such a magnitude as to produce permanent damage in the structure, provided the maneuvering valves worked, can you tell me from your figures first whether the failure of any one maneuvering valve to work would have sufficiently increased the gas pressure as to render probable permanent damage to the structure, and, second, what average percentage of reduction of lift of all maneuvering valves would have been necessary to so increase the stresses as to render probable some permanent damage to the structure? A. I think, sir, that any one maneuvering valve absolutely failing to open, staying tight shut, would probably have burst that cell, or at least damaged the structure over that cell. I should not have said "burst the cell," because that is a bad term. You might have got actual parting of the fabric, but that is not very probable. In regard to the reduction in discharge of all the valves to actually break the ship ——

Q. (Interposing.) Not to actually break the ship, but sufficiently raise your calculated stresses to a point where permanent injury to some part of the structure might have resulted? A. I have made no calculations on that subject, so I speak subject to further calculations, but I think that they could easily have withstood a 25 per cent or even a 50 per cent reduction in discharge capacity before those forces alone would have overstrained the ship, but I would like to make some calculations on that question, sir.

Q. You felt, however, from your general knowledge of the subject, reasonably confident that the percentage of the reduction would have been reasonably within your statement? A. Yes, sir.

Q. Within those limits, between 25 and 50 per cent? A. Yes, sir.

Q. So that it would have required a lost motion between the point of application in the control car and the valves themselves of something in the nature of two and one-fourth to four and one-half inches? A. Yes, sir.

Q. Out of the total of nine inches? A. Yes, sir.

Q. To have produced such stresses? A. Yes, sir.

Q. I would like to get clear the three elements which I believe enter into the total stresses at the time of the last violent rise which might have produced a break in the ship? A. The bending moment was one, the primary bending of the ship.

Q. Due to aerodynamic force? A. Yes, producing an end load on longitudinal girders. Secondly, the distortion of the intermediate transverse frames out of the straight line between the main frames. The intermediate frames have no wires, and they rise and change shape with respect to the main frames, so that the longitudinal girder has to go over a little curve, so to speak.

Q. May I interject there? Is that what you would term a normal static stress? A. Yes; that is a normal static stress.

Q. And the third element? A. And the third element is the load on the longitudinal, acting as a beam between main and intermediate frames. That is the gas pressure load applied directly on the base of the longitudinal and by the netting wires. The outer cover I omitted, because that helps out the cause. It acts against the other loads.

Q. As I understand you, two of those, the static forces, are always present? A. Yes, sir.

Q. Then that being the case from your calculations — or do your calculations convince you that the *Shenandoah* was broken by the aerodynamic forces that were brought against it? A. Yes, sir.

Q. The aerodynamic forces and those two static forces that are always present? A. Yes, sir.

Q. Will you furnish Professor Hovgaard with the figures showing the relation between the pressures which would be reached if the maneuvering valves' discharge capacity were reduced by 50 per cent, compared to the pressure which would be reached in a particular cell in which the maneuvering valve remained entirely closed? A. I will.

Q. Do you think it likely, Mr. Burgess, that a pure shear force would have been capable of producing overstrains, or even breaking the ship under the conditions of the loss of the *Shenandoah?* A. No, sir. I do not think it seems probable. I think it is something that cannot be eliminated as totally impossible. I think it is possible, but it does not seem nearly as likely as bending due to aerodynamic forces under the conditions I have already presented.

Q. Speaking of the gas pressures at the time when the ship was at the maximum altitude you mentioned that due to certain information concerning resistance in the manifold you found that the calculated pressure would probably be reduced from the something like 10.2 to about 8 per square foot. Upon what information was that based, and what was exactly the reason for that assumption, that you could reduce resistance? A. Last month I gave the calculations showing that during the five minutes in which the hand valves were open, a pair of adjacent cells, one with a hand valve and the other with an automatic valve, would together be deflated by the hand valve alone to 1.7 per cent average — and I assumed that, owing to the resistance of the manifold, instead of taking that 1.7 per cent equally out of each cell, you would take about 3 per cent, or 3.4 per cent, out of one cell and let no gas out of the other cell which had the automatic valve. But note that the action of the hand valve under these assumptions was that when the hand valve is open it automatically would close the automatic

valve, because it would take enough gas out of both cells to reduce the gas pressure below the blowing point of the automatic valves. But, now on receiving further data on the resistance of the manifold, I do not think that opening the hand valve would cause all of the gas which has to leave the cell in five minutes to flow through the manifold. Some of it would go out through the automatic valve, because the resistance of the manifold would increase the gas pressure in the cell with the automatic valve above the blowing point of that valve, so that the total amount of gas taken out of the two cells is actually increased by the resistance or by tying off the manifold. It seems strange, but it is true.

Q. Is that assumption based on actual tests? A. Actual tests on a curve which I received from Lieutenant Whittle.

Q. You testified that the *Shenandoah* was strengthened by an increase in her structural weight. Was that done in such a manner as to affect her airworthiness? A. Yes, sir. There were two principal sources of increase of purely structural weight. One was upon the bow, for the strength of the bow mooring. That, of course, did not increase her airworthiness; but also all of the lower secondary longitudinals were stepped up in dimensions, which I think made her a good deal stronger and more airworthy.

Q. Mr. Burgess, did the designers know of this so-called additional weight which was put on the *Shenandoah?* A. Yes, certainly.

Q. Then, strictly speaking, is it proper or correct for any one to make the statement, in view of the fact that the designers put this extra weight in her, that the *Shenandoah's* structure was greater than her designed weight? A. Oh, yes, it was. I mean, it was 3.6 per cent greater. That is, the returned weight came out 3.6 per cent in excess of the estimated weight.

Q. But the designers agreed to this additional weight, did they not? A. In other words, the designers guessed wrong by 3.6 per cent. It was a pretty good guess for the first ship.

Q. Then the weight that actually turned out was really the designed weight, wasn't it? A. No, I won't say that. It was 3.6 per cent more than the estimated weight.

Q. I understood you to say that the actual weighed on weight of the hull structure of the *Shenandoah* exceeded the designed

weight by only 3.6 per cent? A. No, sir, that was the total fixed weight. That figure applies to total fixed weight, power plant and everything.

Q. In the hull structure, then, how much did the actual weighed on weights of the hull structure exceed the designed weights, if they exceeded them at all? A. I would have to look it up. I could not tell you, sir.

Q. Can you give some idea? A. I do not think they exceeded it at all, because I am pretty sure the power plant ran heavy. If the power plant ran heavy and the whole ship was only 3.6 per cent, the hull must be very close indeed. I do not think it was very different.

Q. Taking the total weighed on weight over the designed weight, it was 3.6? A. Yes, sir; power plant and everything.

CALCULATIONS MADE BY C. P. BURGESS.

It seems reasonable to assume that the transverse aerodynamic force upon the airship when struck by the upward gust which broke her was distributed upon the forebody in proportion to the rate of change of cross-sectional area (in accordance with Munk's theory) and was opposed by the mass inertia of the ship distributed in proportion to the cross-sectional area.

Let F = the total transverse air force.

Let L = the distance from the resultant of F to the center of buoyancy.

Let w = the weight at a main frame; and all weights are assumed to be concentrated at these frames.

Let X = the distance of main frame from the C. B.

Let α = the translational acceleration.

Let β = the angular acceleration.

The inertia force at each frame is $w\,\alpha + w \times \beta$

And the total force and moment are given by:

$$F = \alpha \Sigma w \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$FL = \beta \Sigma w X^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Since w is assumed to be proportional to the cross-sectional area A, equations (1) and (2) may be replaced by:

$$F = C_1 \Sigma A \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$FL = C_2 \Sigma AX \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The C. B. of the *Shenandoah* is at frame 105. $\Sigma A = 6566$ in^2, and $\Sigma AX^2 = 13{,}694{,}700$ m^4, as calculated in Design Memo No. 34.

Table 1 herewith shows the calculation of F and FL when the transverse aerodynamic forces are such that

$$\frac{\triangle F}{\triangle X} = 10 \frac{\triangle A}{\triangle X}$$

where F is in pounds, and A in (meters)^2. From the table:

$$F = 4521.5 \text{ pounds.}$$

$$FL = 328{,}130\text{m. pounds}$$

Whence $C_1 = 4521/6566 = .689$ pounds per square meter

$C_2 = 328{,}130/13{,}694{,}700 = .024$ pounds per cubic meter

The load at any frame is $(\triangle F + C_1 A + C_2 AX)$ pounds.

Table 2 shows the calculation of the load, shear, and bending moment, frame by frame.

The maximum bending moment and the change of sign of the shear occur at Frame 120, and it is therefore very improbable that the ship failed in shear.

According to Munk's theory, the distribution of the transverse aerodynamic force is given by

$$\frac{dF}{dx} = \frac{dA}{dx} \rho \frac{v^2}{2} (k_2 - k_1) \sin 2\theta$$

where k_2 and k_1 are the coefficients of the "additional mass" transversely and longitudinally, respectively. For the *Shenandoah*, $k_2 - k_1 = .924$. $\rho v^2/2$ is the aerodynamic head, i. e., half the air density times the square of the speed in absolute units. θ is the angle between the longitudinal axis of the ship and the relative wind. When the rapidly falling ship struck the upward gust which broke her, the relative velocity v of the ship to the wind might have been about 80 feet per second, and θ about 45 degrees. ρ must have been about .0021 slugs per cubic foot. Whence

$$\triangle F = .00105 \times 6400 \times .924 \times 1 \times \triangle A$$
$$= 6.2 \triangle A \text{ (where } \triangle A = \text{square feet)}$$

or $$\triangle F = 10.77 \times 6.2 \triangle A$$
$$= 66.8 \triangle A, \text{ where } \triangle A = \text{square meter.}$$

From Table 2, the maximum bending moment is 89, 760 meter pounds.

When $\triangle F = 10 \triangle A$; whence the maximum bending when $\triangle F = 66.8 \triangle A$ is $6.68 \times 89{,}760 = 600{,}000$ meter pounds.

From Table 2 of Design Memo No. 21, the stress in the top longitudinal due to a bending moment of one meter pound is .022 pounds per square inch, whence the stress due to a bending moment of 600,000 meter pounds is 13,200 pounds per square inch.

It may be noted that the most severe aerodynamic bending considered in the design of the ship (Design Memo No. 10) was 198,620 meter pounds, due to 6-degree pitch to the relative wind at a speed of 68 feet per second at 6000 feet altitude.

In a later design memorandum (No. 25) the bending moments were calculated by the methods followed by the Zeppelin Co., assuming maximum forces on the tail surfaces, opposed by the inertia of the hull. These forces were arrived at by assuming the same coefficient of force as used by the Zeppelin Co. (Design Memos 24 and 25). The maximum bending moments calculated by this method were 205,000 meter pounds and 157,000 meter pounds in the vertical and horizontal planes, respectively. The maximum resultant bending moment due to simultaneous bending in both planes is 258,000 meter pounds.

Another criterion of the maximum probable aerodynamic bending moment to be expected is given by Herr Naatz of the Parsival Co. as follows:

$$M = .01\, V^{2/3}\, L\, \rho\, \frac{v^2}{2} \text{ in absolute units}$$

Assuming for the *Shenandoah* that the maximum speed was 52 knots at 4000 feet altitude,

$$M = .01 \times 17{,}400 \times 676 \times .00105 \times 88^2$$
$$= 960{,}000 \text{ foot pounds}$$
$$= 292{,}000 \text{ meter pounds}$$

This last figure is about 50 per cent more than given by the assumed conditions in the original stress calculations, but less than half as much as might have occurred according to the foregoing

calculations of probable conditions when the ship broke. The difference of maximum stress resulting from the difference of 93,380 meter pounds bending moment between the original assumption and Naatz's expression is 2050 pounds per square inch, which is only about 9 per cent of the ultimate strength of the girders.

During the early flights of the *Shenandoah*, the aerodynamic stresses were indicated by strain gauges, installed principally at frame 70, where the maximum bending moment from forces applied through the tail surfaces was to be expected. The heaviest stresses observed were when crossing the Allegheny Mountains in the flight to St. Louis on 1 October, 1923. The conditions were brilliant sunshine and a fresh northwest wind blowing across the mountains, causing very rough air. The stresses are recorded in Design Memo No. 27. The maximum range of stress was about 10 per cent less than by Naatz's expression.

ANGULAR ACCELERATION.

The angular acceleration, β, of the airship due to the moment FL, is given by

$$\beta = \frac{FLg}{I}$$

where I is the moment of inertia of the virtual mass of the airship. From Design Memo No. 34, $I = 6.6 \times 10^9$ square foot pounds in air of 90 per cent standard sea level density, allowing 85 per cent for the additional mass in rotation (N.A.C.A. Report No. 184); and $FL = 6.68 \times 3.28 \times 328{,}130 = 7{,}200{,}000$ foot pounds. Whence

$$\beta = \frac{7.2 \times 10^6 \times 32.2 \times 57.3}{6.6 \times 10^9}$$

$$= 3.52 \text{ degrees per second per second.}$$

Such a great angular acceleration would greatly increase the angle of attack, and is in accordance with the statements of survivors that the motion of the ship resembled that of an airplane starting a loop.

The point to be emphasized here is that when the airship is struck by an exceptional transverse gust upon the bow, the initial

angle of attack of the gust may be greatly increased by the ship turning away from it with much greater angular velocity than when turning by rudder action in still air. It is estimated that the greatest possible angular velocity of the *Shenandoah* in still air is about 2.2 degrees per second, and the maximum angular acceleration about 0.6 degrees per second per second.

VERTICAL ACCELERATION.

To change the velocity from 2 meters per second up to 5 meters per second down, a total change of 7 meter seconds or 23 feet per second within 60 seconds, the force required is given by

$$F = V_m \frac{dv}{dt}$$

V_m = virtual mass = $2 \times .002 \times 2{,}230{,}000 = 8920$ slugs

(1 slug = g pounds = 32.2 pounds)

density of air = .0644 pounds per cubic foot = .002 slugs per cubic foot.

$$\frac{dv}{dt} = \frac{23}{60} = .383 \text{ feet per second per second}$$

$$F = 8920 \times .383 = 3420 \text{ pounds}$$

Compare $F = 6.68 \times 4521 = 30{,}200$ pounds.

Evidently the downward acceleration shown by the barograph record was not serious.

TABLE I.

Aerodynamic Force and moment on Bow.

Station	△A m²	△F Pounds	Arm Inches	Moment Inches, Pounds
130	2.36	23.6	25	590
140	8.64	86.4	35	3020
150	28.25	282.5	45	12,720
160	48.7	487	55	26,800
170	77.0	770	65	50,000
180	108.2	1082	75	81,300
188	102	1020	83	84,600
194.75	77	770	89.75	69,100
		4521.5 = F		328,130 = FL

TABLE 2.

Load, shear and bending.

Station	△A m²	△F lbs.	A m²	C_1A lbs.	AX m³	C_2AX lbs.	Load lbs.	Shear lbs.	B. M. m. lbs.
0			24	16.5	— 2,520	— 60.5	— 44	— 44	0
10			75	51.6	— 7,125	—171	—119	— 163	— 440
20			140	96.5	—11,900	—276	—180	— 343	— 2070
30			210	144.8	—15,750	—378	—233	— 576	— 5500
40			280	193	—18,200	—437	—244	— 820	—11,260
50			343	236	—18,900	—454	—218	—1038	—19,460
60			392	270	—17,630	—424	—154	—1192	—29,840
70			428	295	—15,000	—360	— 65	—1257	—41,760
80			447	308	—11,200	—269	39	—1218	—54,330
90			452	312	— 6,800	—163	149	—1069	—66,510
100			452	312	— 2,260	— 54	258	— 811	—77,200
110			452	312	2,260	54	366	— 445	—85,310
120			452	312	6,800	163	475	30	—89,760
130	2.36	— 23.6	452	312	11,300	271	559	589	—89,460
140	8.64	— 86.4	448	308	15,700	376	598	1187	—83,570
150	28.25	— 282.5	429	295	19,300	463	475	1662	—71,700
160	48.7	— 487.0	392	270	21,600	519	302	1964	—55,080
170	77.0	— 770	330	227	21,450	515	— 28	1936	—35,440
180	108.2	—1082	236	163	17,700	425	—494	1442	—16,080
188	102	—1020	132	91	10,950	263	—666	776	— 4,550
194.75	77	— 770	0	0	0	0	—770		690
	—4125.5	6566	4525						

Maximum shear is

$$6.68 \times 1964 = 13{,}100 \text{ pounds,}$$

giving a maximum stress in the shear wires of

$$2 \times 13{,}100 \times .0771 = 2020 \text{ pounds.}$$

Strength of wire is 2500 pounds.

Maximum occurs at frame 160.

GAS PRESSURE STRESSES.

1. RISE OF 1760 FEET IN SEVEN MINUTES.

It is assumed that the U.S.S. *Shenandoah* rose 1760 feet above pressure height in seven minutes, at a nearly constant rate of 250 feet per minute. During five of these seven minutes the hand valves were held open. The gas pressure at the end of this rise will be investigated under two assumptions, as follows:

(*a*) That the hand valving occurred during the first five of the seven minutes.

(*b*) That the hand valving occurred during the last five of the seven minutes.

On the assumption that the gas expands 3.3 per cent per 1000 feet rise, the gas to be discharged per minute from each parallel middle body cell is $250 \times 150 \times .033 = 1240$ cubic feet. According to the gas cell valve test carried out by Lieut. Whittle, the discharge from one hand valve when wide open at 4000 feet altitude is 4400 cubic feet per minute, but in accordance with my previous testimony to the Court, it will be assumed that the actual discharge was 3200 cubic feet per minute per valve, corresponding to 73 per cent full open. The discharge from the hand valve is $3200 - (2 \times 1240) = 720$ cubic feet per minute more than required from a pair of cells, so that if the manifold fully equalized the pressure there would be no discharge from the automatic valves.

The test of the gas manifold discharge capacity carried out by Lieut. Whittle showed that a differential pressure of about 18 millimeters between two adjacent cells is required to discharge 1240 cubic feet per minute through the manifold; but at this pressure the automatic valve would be discharging about 2000 cubic feet per minute. It follows that during the ascent, the resistance of the manifold would be sufficient to keep the cell with the automatic valve at pressure height, 13 millimeters, blowing a little gas, probably about 240 cubic feet per minute, through the automatic valve, while the other 1000 cubic feet per minute passes by the manifold or the bulge of the cell ends into the cell with the hand valve. The net result is to discharge $3200 - 1000 = 2200$ cubic feet per minute from the cell with the hand valve, whereas only 1240 cubic feet per minute is required to be discharged. This cell will, therefore, be deflated $2200 - 1240 = 960$ cubic feet per minute, or 4800 cubic feet in 5 minutes, corresponding to 3.2 per cent of its volume, leaving the cell 96.8 per cent inflated.

During the subsequent rise of 500 feet with the hand valves closed, the inflation of the hand valve cells would increase only 1.7 per cent to 98.5 per cent.

If, according to the second assumption, which is Professor Hovgaard's interpretation of the evidence and the barograph record, the ship rose 500 feet in two minutes above pressure height before the hand valves were opened, an automatic valve would have to discharge 2480 cubic feet per minute for a pair of midship gas cells. A pressure of about 19 millimeters is required for this rate of discharge.

During the final two minutes of the seven minutes' ascent, the hand valves are deflating their cells to 96.8 per cent inflation. It will thus be seen that the second assumption is the more favorable one to the ship because of the greater total discharge from the automatic valves.

2. RISE OF 700 FEET IN 42 SECONDS.

It is assumed that the ship rose 700 feet in 42 seconds with hand valves closed, following the 1760-foot rise in 7 minutes. At the beginning of this rise, the cells with automatic valves were at pressure height. The midship cells with hand valves were either 98.5 per cent full according to assumption (*a*) above or 96.8 per cent full according to the more probable assumption (*b*). During a rise of 700 feet each cell must discharge $.033 \times 700 \times 150 =$ 3460 cubic feet, or expand 2.3 per cent. By assumption (*a*), 4670 cubic feet must be discharged through the automatic valve. By assumption (*b*), it is assumed that no gas is discharged from the hand valve cell, leaving 3460 cubic feet to be discharged through the automatic valve. These rates of discharge are 6660 cubic feet per minute, corresponding to a pressure of 27 millimeters at the automatic valve by assumption (*a*), or 4950 cubic feet per minute, and 24 millimeters pressure by assumption (*b*).

3. DEFLECTION OF END OF GAS CELL.

During the final rapid rise of 700 feet in 42 seconds, the comparatively slow equalizing effect of the manifold need not be considered because of the much more rapid equalization by the bulging of the main frame wires between adjacent cells.

The maximum deflection of an initially taut wire subjected to a uniformly distributed transverse load is given by:

$$Y^2 = 3\ L^2\ T/8\ Ea$$

From the gas cell test, Design Memo No. 35, it may be assumed for mean values that:

$L = 50$ feet
$T = 485$ pounds
$E = 20{,}000{,}000$ pounds per square inch
$a = .0095$ square inch

$$Y^2 = \frac{3 \times 2500 \times 485}{8 \times 20{,}000{,}000 \times .0095} = 2.4 \text{ square feet}$$

$$Y = 1.55 \text{ feet.}$$

This is the maximum deflection of the wiring. Allowing for the bulge of the fabric between the wires, it is believed conservative to allow 1 foot mean bulge, or sufficient to increase the volume of the cell 3 per cent by the bulge of each end under a differential pressure of 12 millimeters. Therefore, a rise of 1000 feet above pressure height with the hand valve closed may be taken care of by the bulge of the cell into its neighbor fitted with an automatic valve.

4. STRESSES IN THE LONGITUDINALS.

Comparison of the foregoing gas pressure calculations with those previously submitted to the Court shows that the resistance of the manifold to equalization of pressure between cells reduces the maximum pressure in the cells. This is due to the fact that because of the resistance of the manifold, the opening of the hand valves does not cause the automatic valves to close.

In calculations submitted with my previous testimony it was shown that 24 millimeters pressure at the automatic valve, or 7.96 pounds per square foot at the top of the ship produces a stress of —10,400 pounds per square inch in the apex channel of the C longitudinal. The new calculations taking into account the experimentally determined resistance of the manifold, show maximum pressures of 24 to 26 millimeters instead of 34.9 millimeters, as previously estimated at the height of the automatic valves.

5. PRESSURE AFTER FALLING 3000 FEET.

In answer to previous hypothetical questions it was assumed the ship was 100 per cent full of gas lifting .049 pounds per cubic foot at a maximum altitude of about 6000 feet. After falling

3000 feet, she would be 90 per cent full of gas lifting .0545 pounds per cubic foot. The level of zero pressures would be approximately 65 feet below the top of the ship. The pressure at the top is, therefore, $65 \times .0545 = 3.54$ pounds per square foot. Proceeding as in previous calculations:

$P = 14.15 \times 3.54 = 50$ pounds. $P^{2/3} = 13.6$

$T = 17.2 \times 13.6 = 234$ pounds

$\sin \theta = 8.92 \times 3.54/234 = .135$

$\theta = 7$ degrees, 46 minutes

$\psi = 94$ degrees, 21 miuutes $-$ 7 degrees, 46 minutes $=$ 86 degrees, 35 minutes

$\cos \psi = .0596$.

Radial load from netting $=$

$.975 \times 234 \times .0596 = 13.6$ pounds per foot

Direct load $= 1.04 \times 3.54 = 3.7$ pounds per foot

Total load $= 17.3$ pounds per foot

Corresponding stress in C longitudinal is

$-17.3 \times 269/1.46 = -3190$ pounds per square inch.

Adding —4090 pounds per square inch for the distortion of the intermediate frame, and assuming that the outer cover was slack, the total stress at the joints due to the gas alone was —7280 pounds per square inch. Adding this to the —13,200 pounds per square inch due to the primary bending gives —20,480 pounds per square inch. Experiments with strain gauges have shown that the stresses are irregularly distributed among the girders, and the stress in any one girder may be 25 per cent more than calculated. Adding 25 per cent to the total calculated, for the C longitudinal gives —25,600 pounds per square inch, which is near the failing load of —26,000 to —27,000 pounds per square inch at the joint. Since in this case the stress is due in greater part to the uniform end load, it is to be expected that failure would occur in the single latticed portion of the girder where the strength is about —20,000 to —21,000 pounds per square inch. The double latticing is effective about 26 inches each side of the joints; and assuming the shear to be $17.3 \times 8.2 = 142$ pounds, the reduction in bending moment in 26 inches is $26 \times 142 = 3700$ inch pounds, and the reduction in stress in the apex channel is $3700/1.46 = 2540$ pounds

per square inch; making the theoretical stress —20,480 + 2,540 = —17,940 pounds per square inch. Adding 25 per cent gives —22,400 pounds per square inch, or more than the crippling stress of the girder.

6. EFFECT OF INCREASED STRETCH OF NETTING WIRES.

Given p = 34.9 millimeters at valve or
10.21 pounds per square foot at top of ship,
$P = 14.15\ p = 145$ pounds. $P^{2/3} = 27.6$
Let E = 15,000,000 pounds per square inch.
Then $T = 13.6\ P^{2/3} = 376$ pounds
$\sin \theta = 8.92 \times 10.21/376 = .242$
$\theta = 14$ degrees
$\psi = 180$ degrees — 85 degrees 39 minutes — 14 degrees = 80 degrees 21 minutes
$\cos \psi = .1677$
Radial load from netting =
$.975 \times 376 \times .1677 = 61.5$ pounds per foot
Direct load = 10.6 pounds per foot
Total load = 72.1 pounds per foot
Or 72.1 — 64.8 = 7.3 pounds per foot more than when
E = 30,000,000 pounds per square inch.
And the increase of maximum stress is $7.3 \times \overline{16.4}^2/1.46$
= — 1350 pounds per square inch, which is not very important.
If in the 90 per cent inflated case,
E = 15,000,000 pounds per square inch,
$T = 234/1.26 = 186$ pounds
$\sin \theta = .135 \times 1.26 = .170$
$\theta = 9$ degrees 47 minutes
$\psi = 94$ degrees 21 minutes — 9 degrees 47 minutes = 84 degrees 34 minutes
$\cos \psi = .0947$
Load from netting = $.975 \times 186 \times .0947$
= 17.2 pounds per foot
Direct load = 3.7 pounds per foot
Total load = 20.9 pounds per foot

The increase is only $20.9 - 17.3 = 3.6$ pounds per foot, and the increase of stress is only $-3.6 \times 269/1.46 = -664$ pounds per square inch.

7. STRESS AT END OF DOUBLE LATTICING.

Suppose that at the maximum altitude, the gas pressure combined with the distortion of the intermediate frame and the tension in the outer cover produced a maximum stress of —12,950 pounds per square inch as given in my previous testimony. If the outer cover is not effective, and the modulus of the wiring is halved to obtain an extreme case, the stress becomes $-12{,}950 - 2320 - 1350 = -16{,}620$ pounds per square inch at the joint where the ultimate strength should be at least —26,000 pounds per square inch. The shear is $8.2 \times 72 = 590$ pounds, and the change in bending moment in 26 inches is $26 \times 590 = 15{,}350$ in pounds, and the reduction in stress in the C longitudinal is $15{,}350/1.46 = 10{,}500$ pounds per square inch, making the total stress at the end of the double latticing $-16{,}620 + 10{,}500 = -6120$ pounds per square inch. A break due primarily to gas pressure should therefore occur at the joint instead of at the end of the double latticing, as when the bending of the hull is the primary cause of failure.

8. AERODYNAMIC FORCE TO BREAK SHIP AT PEAK OF RISE.

An aerodynamic sagging bending moment of 190,000 meter pounds would increase the stress by —4180 pounds per square inch, which, added to the —16,620 pounds per square inch previously calculated, gives a total of 20,800 pounds per square inch. An addition of 25 per cent makes 26,000 pounds per square inch, or approximately the failing load. Since a gust of 80 feet per second velocity at 45 degrees to the axis produces a bending moment of 600,000 meter pounds, and the force varies as the square of the velocity, the velocity required to produce 190,000 meter pounds bending is $80\sqrt{190/600} = 45$ feet per second, *relatively to the ship*, and at 45 degrees angle of attack.

9. EFFECT OF IMPERFECT OPERATION OF THE HAND VALVES.

In the preceding calculations, it was assumed that the discharge of gas through the hand valves corresponded to 73 per cent open-

ing. With 50 per cent opening the discharge at 25 millimeters pressure is 2500 cubic feet per minute. When rising at 250 feet per minute, a discharge of 2480 cubic feet per minute is required from each pair of midship cells. The difference in pressure at the levels of the hand and automatic valves is 15 millimeters, so that 25 millimeters at the hand valve corresponds to 10 millimeters at the automatic valve, or just above the opening pressure, giving a barely perceptible discharge through the automatic valves. It follows that during the five minutes' rise at 250 feet per minute with the hand valves 50 per cent open, both cells will be full with a pressure a little less than 25 millimeters at the hand valves, and rather more at the tops of the cells with the automatic valves.

During the rise of 700 feet in 42 seconds with the hand valves closed, the required discharge from a pair of cells is 9900 cubic feet per minute, requiring a pressure of 31 millimeters at the automatic valves according to Lieut. Whittle's latest report, instead of 34.9 millimeters as found by extrapolation from his earlier report. During this sharp rise, the effect of the manifold may be neglected, and the cells with the hand valves must expand 2 per cent. This expansion is obtained by the bulge of the cell end through the main frame with a differential pressure of about 8 millimeters. The pressure at the top of the hand valve cell is therefore $31 + 8 + 15 = 54$ millimeters $= 11.1$ pounds per square foot.

Assuming an effective E of 15,000,000 pounds per square inch for the gas cell wires, the stresses are calculated as follows:

$$p = 11.1 \text{ pounds per square foot}$$

$$P = 14.15\ p = 157 \text{ pounds.} \quad P^{2/3} = 29.2$$

$$T = 13.6 \times P^{2/3} = 397 \text{ pounds}$$

$$\sin \theta = 8.92 \times 11.1/397 = .25$$

$$\theta = 14 \text{ degrees } 30 \text{ minutes}$$

$\psi = 94$ degrees 21 minutes $-$ 14 degrees 30 minutes $= 79$ degrees 51 minutes

$$\cos \psi = .1745$$

Radial load from netting =
.975 × 397 × .1745 = 67.5 pounds per foot
Direct load =
$\frac{1.04 \times 11.1}{} = 11.5$ pounds per foot

Total.....79.0 pounds per foot

Stress in C longitudinal =
$-79 \times 269 / 1.46 = 14{,}600$ pounds per square inch.

Adding —4000 pounds per square inch for the distortion of the intermediate frame gives —18,600 pounds per square inch. Adding the usual 25 per cent for exceptional stresses gives —23,300 pounds per square inch, which is a critical condition. From this it is concluded that 50 per cent opening of any of the hand valves near the middle of the ship represents a critical condition, and any less opening would probably cause disaster.

C. P. BURGESS.

TESTIMONY OF LIEUTENANT G. V. WHITTLE (C.C.)

Q. Have you worked out the hypothetical problem previously introduced before this court which involved the vertical rise of the *Shenandoah* of 1260 feet above pressure height in five minutes, during which the maneuvering valves were open, followed by a rise of 700 feet in 42 seconds, during which the maneuvering valves were closed? A. I have.

Q. Will you make your solution available to the Court and compare the resulting gas pressures with those that would have occurred with the original arrangements of valves. A. I have worked up this problem, assuming the atmospheric conditions given in the National Advisory Committee Report No. 218, called the "Standard Atmosphere." I started this rise at an altitude of 4400 feet. The 1260-foot rise carried the ship to 5300 feet, and the 700-foot rise above that carried the ship to 6000. I mention that because in developing the rate of expansion of helium between these points, I get a figure of 3.1 per cent instead of 3.3 per cent, which has been previously introduced before this Court. The 3.3 per cent is more severe to the ship and it is not fair, because in my estimation in a rise such as indicated the expansion of the gas cannot be more than 3.1 per cent, but it can be less. I worked

this problem out considering the adiabatic effect of the rise. That is, when an airship ascends the gas cools and the rate of cooling depends upon the rate of ascent. This is a fact well known to experienced airship pilots and is taken advantage of under certain conditions. Now, the adiabatic expanding of helium is considerably higher than that of hydrogen and this fact was always taken advantage of by pilots of hydrogen-filled airships. I figure out that if the rise, adiabatically, and rate of expansion per thousand feet is 2.27 per cent, the rate for temperature equalized expansion is 3.1 per cent, so that in any case of the rise of an airship, the actual rate of expansion must fall between those two limits. If an airship rises very, very slowly, so that the gas can take up heat from the outside, it will approach the 3.1 per cent figure. If the rise were instantaneous, the rate of expansion would be the 2.37 per cent figure. Now, in my solution, I have assumed a rate of expansion half way between the two, which I think is unfair to the ship. I think that the rate of expansion would be more than that because the decrease in temperature in this rise — the rise is of such a relatively short distance — the decrease of temperature is only of the order of 20 degrees or so, and I do not believe, considering the surface and volume of the cells, and the fact that they are protected by an outer cover and all that, much heating could take place in that rise from the outside air.

In working out this problem I have used the best data which is available on the discharge characteristics of the valves and inflation manifold. I have considered the adiabatic effect, as I have said before, but I neglected one consideration which, if included, would favor the ship, and that is that the gas valve will discharge more cubic feet of gas at an altitude than it will at sea level. I have neglected that consideration because I do not believe it affects the problem much one way or the other, but still if it were included it would favor the ship.

The results I get are — I considered a pair of parallel cells 13 and 14 — at the end of the 1260-foot rise, I get a pressure of 12.7 millimeters of water in cell 13, and in cell 14, which is equipped with maneuvering valve, I get no pressure at all. That is, the discharge through the maneuvering valve was greater than the volume manufactured by the rise of the ship. At the end of the

1260 plus 700-foot rise, I get a pressure of 20.6 millimeters in the cell fitted with the automatic valve and still no pressure in cell 14. This condition will repeat itself through the ship.

Not only do I get no pressure in cell 14 at the very peak of the rise, but I find that this cell lacked a considerable amount of being full. I figured that the cell lacked 6217 cubic feet of being full, so that even if the valves discharged were coming down, the pressure I have indicated would not be changed until cell 14 became full; that is, there is a leeway of 6217 feet which does not affect the pressures that I have calculated.

Now, with the original installation of 18 automatic valves and considering the original method of operation, I find that the pressure in any of the parallel cells would be 1.63 millimeters at the end of the 1260-foot rise, and 23.5 millimeters at the end of the 1260 plus the 700-foot rise. In other words, with the modified valve arrangement, as operated in the hypothetical problem, the gas pressures at all points along the rise are less than would have occurred with the original valve installation and the original method of operation for the same problem.

This solution I have worked out in detail and given references and I would like to present it to the Court as evidence.

HYPOTHETICAL PROBLEM ON GAS CELL PRESSURE.

Consider cells Nos. 13 and 14. Cell No. 13 had an automatic valve; cell No. 14 a maneuvering valve. The volume of cell No. 13 was 150,440 cubic feet; that of cell No. 14 was 150,100 cubic feet.

While adiabatic cooling takes place in the 1260-foot rise, the rate of rise is only 252 feet per minute, and the effect of this cooling will be neglected in this portion of the rise.

From N.A.C.A. Report No. 218, "The Standard Atmosphere":

For 4040 feet, $P = 25.80$ inches, $T = 504$ degrees
For 5300 feet $P_1 = 24.61$ inches, $T_1 = 499.5$ degrees
Let PVT be the conditions at 4040 feet
and $P_1V_1T_1$ be the conditions at 5300 feet.

$$\text{Then as } PVT_1 = P_1V_1T \quad V_1 = \frac{PT_1V}{P_1T} = \frac{25.8 \times 499.5\ V}{24.61 \times 504}$$
$$= 1.039\ V$$

Hence the increase in volume is 3.9 per cent for 1260 feet or 3.1 per cent per 1000 feet. Therefore the respective increases in volume of cells Nos. 13 and 14 for a 1260-foot rise are:

For cell No. 13, 3.9 per cent of 150,440 or 5867 cubic feet for 5 minutes.

For cell No. 14, 3. 9 per cent of 150,100 or 5854 cubic feet for 5 minutes.

Consider cell No. 13: 1173 cubic feet of helium would be discharged per minute through the inflation manifold and the automatic valve, divided as follows:

Through the automatic valve, 200 cubic feet per minute (12.7 millimeters' pressure); through the inflation manifold into cell No. 14, 973 cubic feet per minute (9.9 millimeters' pressure). This division of the escape of gas from cell No. 13 is predicated on the Lakehurst tests of the *Shenandoah's* inflation manifold and gas valves, on the assumption that a pressure difference of 2.8 millimeters of water exists between the inflation manifold and the automatic valve due to the difference in their vertical positions. This gives a pressure at the automatic valve of 12.7 millimeters of water.

Now for the 700-foot rise in 42 seconds, we will take the 3.1 per cent increase in volume per 1000-foot rise to hold for normal expansion, but will work out the adiabatic rate of increase in volume. For helium expanded adiabatically, the general gas equation becomes $PV^{1.67} = P_1V_1^{1.67}$, and to make a direct comparison with the normal figures deduced previously, we will use the standard atmosphere conditions for pressure at the 4040 and 5300-foot altitudes.

$$\text{Then } V_1^{1.67} = \frac{PV^{1.67}}{P_1}, \text{ and assuming } V = 1.$$

$$1.67 \log V_1 = \log P - \log P_1$$

$$\log V_1 = \frac{\log 25.8 - \log 24.61}{1.67} = .01228$$

$$V_1 = 1.0287.$$

Therefore the increase in volume for a 1260-foot rise adiabatically is 2.87 per cent or 2.27 per cent per 1000-foot rise. The adiabatic heating and cooling of hydrogen is a condition well

known to experienced airship pilots, and is often taken advantage of under certain operating conditions. As the adiabatic exponent for helium is 1.67 as compared to 1.419 for hydrogen, it is evident that the adiabatic effect is greater for helium than for hydrogen.

Now as the normal (temperature equalized) expansion rate for helium as developed previously is 3.1 per cent per 1000-foot rise, and for adiabatic expansion is 2.27 per cent per 1000-foot rise, it is evident that in any case the actual rate of expansion of helium in an airship will fall between these two figures. Now in the case of an airship rising at the rate of 1000 feet per minute, which is a rapid rate of rise, it seems reasonable to take a figure of at least 2.68 per cent for the rate of expansion of helium per 1000 feet, which figure is halfway between the normal rate and the adiabatic rate. Still considering cell No. 13, for the last 700-foot rise from 5300 feet to 6000 feet, the volume in the cell increases at the rate of 2.68 per cent per 1000 feet, which gives a rate of increase in volume per 1000-foot rise of this cell of 2.68 per cent of 150,440 or 4032 cubic feet. Part of this gas would be discharged through the automatic valve and part through the inflation manifold into cell No. 14, as follows:

	Rate per 1000 ft.	*Actual volume for 700-ft. rise*	*Pressure at each*
Through automatic valve.....	2700	1890	20.6 mm.
Through inflation manifold to cell No. 14.....	1332	932	17.8 mm.
Total	4032	2822	

Consequently, at the end of the 1260 plus 700-foot rise, the pressure in cell No. 13 at the valve is only 20.6 millimeters of water.

Now consider cell No. 14: For the 1260-foot rise from 4040 to 5300 feet, the volume increases as follows: through the inflation manifold from cell No. 13, $973 \times 5 = 4865$ cubic feet; due to the rise (normal expansion), 3.9 per cent of 150,100 or 5854 cubic feet, a total increase of 10,719 cubic feet. The decrease in volume due to the operation of the maneuvering valves (from Lakehurst valve test data) with 19.5 millimeters' pressure at the valve is $5 \times 4100 = 20{,}500$ cubic feet without making correction for the reduction in outside pressure due to the altitude above sea level. Therefore cell No. 14 lacks $20{,}500 - 10{,}719$, or 9781 cubic

feet of being full at the end of the 1260-foot rise. For the next and last rise of 700 feet, the maneuvering valve is closed. The volume of this cell is increased as follows:

Gas from cell No. 13 through inflation manifold, 932 cubic feet.

Increase in volume due to 700-foot rise (.7 × 2.68 per cent of (150,100 — 9781), 2632 cubic feet.

Total increase, 3564 cubic feet.

Therefore at the end of the 1260 plus 700-foot rise, cell No. 14 lacks 6217 (9781 — 3564) cubic feet of being 100 per cent full.

With the original installation of 18 automatic valves and the original method of operation, the pressure which would occur in the gas cells due to a rise as given above will now be determined.

As all of the larger cells were equipped with automatic valves, we will consider cell No. 13 only. The 1260-foot rise from 4040 to 5300 feet altitude would cause a rate of increase in volume per minute of 1/5 of 3.9 per cent or .78 per cent per minute. This rate gives an increase in volume of .78 per cent of 150,440, or 1173 cubic feet per minute. This would give an equilibrium pressure of 16.3 millimeters of water at the automatic valve.

The next rise of 700 feet in 42 seconds corresponds to a rate of rise of 1000 feet per minute. The rate of increase in volume per 1000 feet would be about 2.68 per cent, considering the adiabatic effect. 2.68 per cent of 150,440 = 4032 cubic feet, which from the Lakehurst valve data gives an equilibrium pressure of about 23.5 millimeters of water at the automatic valve. In the original valve installation, as all of the large cells had automatic valves, this calculation would apply to all of the parallel body cells.

G. V. WHITTLE.

BUREAU OF STANDARDS REPORT

ON TESTS OF MATERIAL FROM THE U.S.S. "SHENANDOAH," SUBMITTED BY THE "SHENANDOAH" COURT OF INQUIRY OF THE U. S. NAVY.

SUMMARY.

The samples of material submitted have been minutely examined, visually, microscopically and by means of physical tests, to determine whether any deterioration existed in the material which might have weakened the structure of the ship.

No single specimen was found which had deteriorated sufficiently to lower the resistance of the structure of the ship to static or aerodynamic loads, nor was any evidence found that fatigue failure had contributed to the wreck of the ship.

TESTS ON DURALUMIN PARTS.

Two hundred and nineteen bend tests, twenty-nine tensile tests, twenty repeated bend tests were made on channel material, and forty-nine tensile tests on lattices. Sixteen comparisons of the modulus of elasticity in compression and tension, and three determinations of the value of the modulus of elasticity in tension were made on channel material from the ship.

Micrographic examinations were made of two hundred and eleven specimens selected on the basis of visual inspection or physical tests as most likely to be deteriorated by corrosion.

Fatigue tests are in progress, but no definite time can be set at which they will be completed.

TESTS ON SHEAR AND NETTING WIRES.

Nine tensile tests were made on shear and netting wires, three of which were broken in the wreck. As these showed uniformly high quality, no further tests were considered necessary.

TESTS ON CONTROL SPROCKET CHAIN.

Two breaking tests were made on a bronze control sprocket chain. The two tests gave a breaking strength of 2025 and 2030 pounds, respectively, the failure being by shear through the rivets.

CONCLUSIONS.

Intercrystalline corrosion of about the same extent as was observed in the investigation made for the Bureau of Aeronautics last spring was found to be general in the duralumin channels. This corrosion appeared to be distributed in random patches over the surfaces of the channels, with no indication of especially severe attack in any special parts. In general, the corroded patches formed only a small portion of the total area of the surface. The maximum depth of a corroded spot found was eight-thousandths of an inch, and on the average was from one to two-

thousandths of an inch. Of the tensile specimens chosen from material presumably most severely corroded, one showed a tensile strength of 45,200 pounds per square inch, while the others ranged from 53,500 to 63,200 pounds per square inch. Bureau of Standards tests on comparable new material gave results varying from 55,000 to 65,000 pounds per square inch. The percentage elongation in two inches varied from 5 per cent to 17½ per cent, where comparable new material varied from 18 to 24 per cent.*

Intercrystalline corrosion was found in the lattices in smaller degree than in the channels. Corrosion of lattices from the *Shenandoah* had not been found in the previous investigation, where fewer specimens were examined.

The tests showed the modulus of elasticity of the corroded material to be the same in tension as in compression and to have the same value in the corroded as in the uncorroded material.

Since the duralumin girders of the *Shenandoah* were constructed so that they would fail by flexure within the elastic limit of the material without producing tensile stresses in any case of more than 36,050 pounds per square inch, as found by tests, none of the material found in this thorough search had corroded sufficiently to lessen the resistance of the ship to static or aerodynamic forces.

No results on fatigue tests of specimens from the wreck are as yet available, but tests on more severely corroded material from a scrap heap were made last spring.* These tests were made at relatively high stress ranges and showed a lowering of the stress range which the material could endure for a given number of vibrations to from 60 to 80 per cent of that for uncorroded material.* However, one specimen was removed unbroken from the machine after fourteen million four hundred and thirty-one thousand alternations at a stress range of 27,200 ($\pm$ 13,600) pounds per square inch.

An examination with a hand lens was made of all breaks found in the material at Lakehurst and an especially careful examination was made of samples No. 14 and No. 17, said to have been subject to vibration, but no sign of a fatigue break was found.

* See Bureau of Standards Supplementary Report, page 628.

This investigation, therefore, furnishes no evidence that fatigue failure contributed to the wreck of the ship.*

IMPORTANCE OF CORROSION OF DURALUMIN.

These tests indicate that at the time of the wreck the corrosion of the duralumin in the *Shenandoah* had not proceeded far enough to weaken the structure of the ship.

It is evident, however, that the material itself was deteriorating and that in time the deterioration due to corrosion would have become so great as to render the ship useless.

At the present time no method is known of preventing this corrosion; the protective coatings used merely delay its action.

That the structure of the *Shenandoah* was not weakened by the deterioration now present in the material was due to the fact that its design did not utilize more than one-half the strength of the material. If in the future the design of such light and flexible girders shall be improved so that the full strength of the material when new can be utilized any deterioration whatever will reduce the factors of safety of the structure. The problem of completely preventing the deterioration of duralumin by corrosion is, therefore, of great importance to the future usefulness of this material.

DETAILED REPORT OF THE TESTS.

On September 30, 1925, a representative of the Bureau of Standards went to Lakehurst to inspect the material from the wreck of the *Shenandoah*, and select material for further test. The material was carefully examined with a hand lens, an especial search being made for evidence of fatigue failure, brittle breaks at bends or obvious corrosion in the duralumin parts.

No breaks having the characteristics of a failure by fatigue were found.

All the breaks showed the typical diagonal shear failure found in tensile or tensile impact specimens.

Some bends, however, showed the characteristic multiple break previously found in material known to have surface corrosion, and some few specimens with obvious surface corrosion were also found.

* See Bureau of Standards Supplementary Report, page 628.

The greater number of these were found in the specimens already selected by a representative of the Naval Aircraft Factory, but several others were added.

Search was also made for shear and netting wires which had broken in the failure. Two pieces were found which showed tensile breaks and one which showed a bend break. These, with some adjacent wires, were selected for test.

The material selected was shipped from Lakehurst on October 7, and received at the Bureau of Standards on October 12.

On October 14 a conference was held between representatives of the Court of Inquiry, Naval Aircraft Factory, the Bureau of Aeronautics and the Bureau of Standards.

The previous investigations of the Bureau of Standards upon the material in the ship were used as a basis for the discussion.

These consisted of the tests on the duralumin girders made for the Bureau of Aeronautics during the construction of the ship,* the tests on fatigue of duralumin, made for the Bureau of Aeronautics and the National Advisory Committee for Aeronautics during the years 1920 to 1925, the results of which are contained in the monthly reports of this investigation, and finally on the investigation of corrosion of duralumin commenced last spring and now carried on the funds of the National Advisory Committee for Aeronautics, the Bureau of Aeronautics and Army Air Service, the results of which are contained in the monthly reports of this investigation and in various special reports.† The tests on the girders had shown that "the ultimate loads are determined solely by the modulus of elasticity of the material and the geometrical dimensions of the girders. The ultimate strength or yield

* Bureau of Standards Report on Test of duralumin girders for Fleet Airship No. 1.—VII-1/Tsm-31556 Lab. No. 0622a21, January, 1923.

† In particular—

Bureau of Standards reports to the Bureau of Aeronautics.

VIII-1/Tm-42032	M.L. No. 5886	December 1, 1924.
VIII-1/Tm-42532	M.L. No. 5959	February 2, 1925.
VIII-1/Tm-42532	M.L. No. 5959	March 13, 1925.
VIII-1/Tm-.....	M.L. No. 5978	April 3, 1925.
VIII-1/Tm-43032	M.L. No. 5991	April 30, 1925.
VIII-1/Tm-42532	M.L. No. 5959	May 18, 1925.

The results of these are summarized in a report on "Embrittlement of Duralumin," May 14, 1925, prepared for and at the request of the Bureau of Aeronautics, and were the basis for the comprehensive study of the subject initiated by the Bureau of Aeronautics now being carried on according to the Bureau of Standards "Outline for Study of Intercrystalline Embrittlement of Duralumin," June 5, 1925.

Reference is also made to—

R + M. (B.A.C.A.) No. 970, Appendix II October, 1924, Note on the Possible Gradual Deterioration of Duralumin, by Prof. C. F. Jenkins.

point of the material either in compression or tension have no influence on the strength so long as they are greater than the critical stresses. With two exceptions (5 meter beam tests 52-6 and 55d-3), the computed maximum stresses were never higher than St = 33,090 pounds per square inch (10m55a, Table 6), in tension, and Sc = 29,660 pounds per square inch (4m72a, Table 6), in compression."* The 55d-3 girder was a special experimental girder not used in the ship. In the 5m 52-b beam test (Report, Table 6), the maximum tensile stress was St = 36,050 pounds per square inch.

The tests also showed that "the lattice stresses measured with the strain gauges were always low and seldom more than one or two thousand pounds per square inch."

The investigation on fatigue had shown that when in good condition fatigue failure of the ship need not be feared. On an assumed complete reversal of stresses of 10,000 pounds per square inch, i.e., a range of stress of 20,000 pounds per square inch, which is much greater than any range of vibratory stress that could reasonably be expected to exist in the ship, fatigue failure would not occur under 10 years (about 9000 hours) of continuous flight at normal cruising speed. (See newspaper release enclosed in letter to Bureau of Aeronautics, December 20, 1923).

The investigation on corrosion had indicated that corrosion of an intercrystalline type such as has also been found by British investigators in duralumin of foreign makes, occurred in the channels of the *Shenandoah* when exposed to the weather, but only infrequently if at all in the lattices. This corrosion was most pronounced in unvarnished material fully exposed to the weather and less in the varnished material in the ship. It was accompanied by a slight decrease in strength, no reduction in yield point and a considerably lowering of the elongation of the tensile test specimens. In cases where considerable corrosion was present there was a characteristic "crazing" of the surface of the specimen. (See photographs following.) A more easily noticed effect was the increase of the radius of curvature at which cracks occurred in a bend specimen.

(* Report on Tests of Duralumin Girders, pp. 8 and 9.)

A relatively few fatigue specimens of badly corroded material taken from a scrap heap had shown * that at high stresses (range of stress change, 35,000 (±17,500) to 50,000 (±25,000) pounds per square inch) the corroded material was considerably less resistant to fatigue than new material, failure occurring at stress ranges varying from 60 per cent to 80 per cent of those required to break the new material† with the same number of alterations of stress, while one specimen withstood fourteen million four hundred and thirty-one thousand alternations at a stress range of 27,200 (±13,600) pounds per square inch.

The tests were accordingly planned to hunt for the parts available in the material which had most severely deteriorated, and determine whether the changes were sufficient to cause any weakening of the structure of the ship.

The following procedure of tests was agreed on as most likely to find any material which had seriously deteriorated:

1. Specimens for metallurgical examination were to be selected from places showing obvious corrosion or brittle bend breaks.

2. Bend specimens were to be taken from every available channel.

3. Micrographic examinations were to be made of selected bend specimens to determine whether the more brittle type of break was definitely associated with corrosion.

4. Tensile and repeated bend specimens were to be taken adjacent to material showing good and poor results in the bend tests to see how closely the tensile results could be predicted from the bend tests.

5. Micrographic examinations were to be made of selected specimens from these tensile and repeated bend tests.

6. A specially careful examination was to be made of specimen No. 18, which had been described as "brittle" by the officers who secured the specimens from the wreck.

7. Measurements of elastic deformation were to be made on a few specimens adjacent to material showing poor bend test results, to determine any possible lowering of the modulus of elasticity due to deterioration.

* See Memorandum on Embrittlement of Duralumin prepared by the Bureau of Standards for the Bureau of Aeronautics, May 14, 1925.

† See Bureau of Standards Supplementary Report, page 628.

8. If time were available, fatigue tests were to be made on the most severely corroded material to confirm the results found in the previous investigation.

9. At the request of the Bureau of Aeronautics, tensile tests were to be made of a sufficient number of the shear and netting wires to determine whether they showed any signs of poor quality.

10. Breaking strength tests were to be made on the sample of control sprocket chain.

The detailed test results follow.

BEND TESTS.

Two hundred and nineteen bend specimens approximately one-half inch by two inches were cut, one from each available channel except from sample No. 18, which was reserved for more thorough tests. A portion near the middle of each specimen was buffed with tripoli, to secure a polished surface on which to observe the cracking. These specimens were clamped in the jaws of long levers (practically eliminating local compressive stresses due to pressure against the mandrel) and bent around a mandrel whose diameter was (within the tolerance of manufacturing) four times the thickness of the specimen. During the bending the specimen was carefully observed with a hand lens. The bending was stopped at the first appearance of a crack and the specimen removed from the jaws. The radius of curvature at the interior of the bend was then measured, and the ratio of radius of curvature to thickness of specimen computed.

Six comparison specimens of each of four thicknesses of uncorroded material, part of the stock furnished under the same specifications for the fatigue investigation were similarly tested.*

The test results are given in Table 1 and plotted in Figure 5. The "Sample Numbers" in Table 1 are the numbers assigned at Lakehurst, and shown in the photographs accompanying the material.

* See Bureau of Standards Supplementary Report, page 628

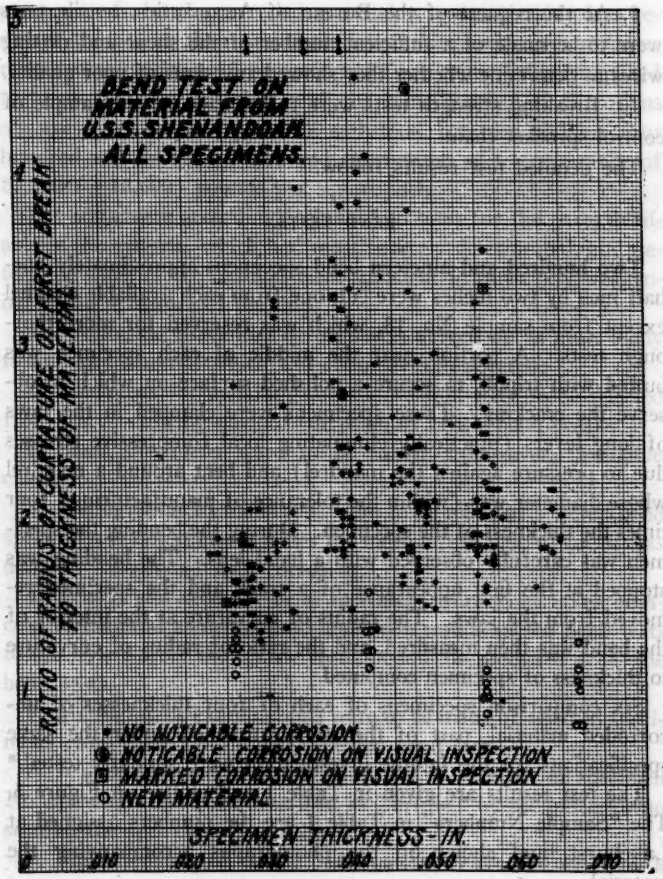


FIGURE 5.—BEND TESTS.

TABLE 1

RESULTS OF BEND TESTS ON DURALUMIN FROM SHENANDOAH AND TENSILE TESTS OF ADJACENT MATERIAL

Piece Number	Specimen Number	Thickness T	Radius R	R/T	Tensile Strength	Elongation in 2 in.	Repeated Reversed Bend
		in.	in.		lb./sq.in.	%	Number
New Material	1	0.026	0.0335	1.29			
"	2		.0350	1.35			
"	3		.0317½	1.22			
"	4		.0297½	1.14	NOTE. See Bureau of		
"	5		.0335	1.29			
"	6		.0365	1.40	Standard's Supple-		
"	7	.043	.0497½	1.18			
"	8		.0680	1.82	mentary Report.		
"	9		.0580	1.38			
"	10		.0580	1.38			
"	11		.05325	1.27			
"	12		.0532½	1.27			
"	13	.056	.0580	1.04			
"	14		.0565	1.01			
"	15		.0507½	0.91			
"	16		.0643½	1.15			
"	17		.0565	1.01			
"	18		.0600	1.07			
"	19	.067	.0702½	1.05			
"	20		.06425	0.96			
"	21		.0642½	0.96			
"	22		.0785	1.17			
"	23		.0885	1.32			
"	24		.0735	1.10			
			SHENANDOAH MATERIAL				
12	116	0.022	0.0415	1.84			
		0.023					
		0.023					
8	82	0.024	.0453	1.93			
12	114		.0433	1.84			
6	69	0.025	.0380	1.55			
9	105		.0430	1.76			
1	5	0.026	.0430	1.69			
8	80		.0410	1.61			
8	83		.0720	2.82			
12	119		.0430	1.69			
12	126		.0380	1.49			
12	134		.0393	1.54			
3	30	0.027	.0430	1.63			
3	31		.0430	1.62			
3	32		.0430	1.62			
6	66		.0430	1.62			
7	73		.0508	1.92			

2. Table 1 (Cont'd)

Piece Number	Specimen Number	Thickness T	Radius R	R/T	Tensile Strength	Elongation in 2 in.	Repeated Reversed Bend Number
		in.	in.		lb./sq.in.	%	
8	81		0.0430	1.62			
9	111		.0410	1.55			
12	117		.0410	1.55			
12	128		.0520	1.96			
12	136		.0410	1.55	59 300	18.0	
3	45	0.028	.0410	1.49	57 800*	6.0	
7	71		.0393	1.43			
8	89		.0490	1.78			
8	96		.0520	1.89			
9	101		.0467$_5$	1.70			
9	108		Huge	Huge			
9	110		.0520	1.78			
12	115		.0410	1.49			
12	120		.0580	2.11			
12	121		.0430	1.58			
12	127		.0467$_5$	1.70			
12	133		.0430	1.56			
12	137		.0445$_5$	1.62			
1	8	0.0290	.0497$_5$	1.75			
3	33		.0392$_5$	1.38	60 400	9.5	
8	91		.0380	1.33	59 600	12.5	
8	97		.0600$_5$	2.10			
12	138		.0467$_5$	1.64			
2	14	0.0300	.0532$_5$	1.81			
3	44		.0430$_5$	1.48			
3	46		.0297$_5$	1.01	61 400	17.5	
3	34	0.0310	.0565	1.85			
7	71A		.0805	2.64			
7	72		.0995	3.28	61 300	12.0	49
12	112		.0980	3.21	55 800*	10.5	86
12	113		.0702$_5$	2.30			
24	185		.0580	1.90			
23	190		.0490	1.61			
7	75	0.0320	.0520	1.85			
7	78		.0445	1.41			
23	192		.0600	1.90			
23	193		.0600$_5$	1.90			
23	194		.0347$_5$	2.69			
3	29	0.0330	.0680	2.09			
3	50		.0490	1.51			
7	74		.0642$_5$	1.98			
3	37	0.0350	Huge	Huge	63 200	12.0	20
3	48	0.0370	.0770	2.11			
12	139	0.0380	.1065	2.84			
20	155		.0702$_5$	1.87			
20	156		.1250	3.33			
20	158		.0885	2.36			
22	180		.0805	2.15			
23	195		.1330	3.55			
3	35	0.0390	Huge	Huge			

* Out of gage marks

3.

Table 1 (Cont'd)

Piece Number	Specimen Number	Thickness T	Radius R	$\frac{R}{T}$	Tensile Strength	Elongation in 2 in.	Repeated Reversed Bend Number
		in.	in.		lb/sq.in.	%	
3	38		0.1330	3.46			
6	63		.0945	2.46			
6	65		.0720	1.87			
6	67		.1250	3.25			
8	70		.1250	3.25			
12	122		.0702[5]	1.83	54 400	15.5	58
12	123		.0735	1.91			
12	125		.0885	2.30			
12	135		.0805	2.09			
20	154		.0945	2.48			
24	168		.1065	2.77			
22	181		.0805	2.09			
31	207		.1485	3.88			
31	208		.0785	2.04			
30	210		.0770	2.00			
30	211		.0770	2.00			
3	47	0.0400	.0987[5]	2.45			
3	53		.0805	2.04			
12	131		.0910	2.30			
12	132		.0770	1.95			
20	157		.0795	2.01			
19	159		.0747[5]	1.89			
24	167		.0800	1.52	58 300	12.5	51
24	169		.1330	3.37			
24	171		.1065	2.70			
23	183		.0847[5]	2.15			
31	206		.1250	3.17			
3	53	0.0410	.1565	3.87			
6	68		.0955	2.36			
12	124		.1640*	4.05*	60 800	12.0	
12	130		.0780	1.93			
24	166		.0940	2.32			
24	172		.1015	2.51			
23	187		Huge	Huge	54 800	7.5	43
23	191		.1875	4.63	60 000	13.0	
24	170	0.0420	.1015	2.45			
23	184		.1720	4.15			
23	188		.1170	2.82			
6	62	0.0430	.0860	2.02			
23	185		.1250	2.94			
23	188		.1095	1.40	53 500	5.0	11
8	95	0.0440	.0795	1.83			
13	143	0.0450	.0995	2.24			
22	182		.1170	2.63			
22	200		.1015	2.28			
1	6	0.0460	.0900	1.98			
1	9		.1095	2.41			
3	40		.1250	2.75			
8	94		.0760	1.67			

* Defect 1/8" long

4. Table 1 (Cont'd)

Piece Number	Specimen Number	Thickness T	Radius R	$\frac{R}{T}$	Tensile Strength	Elongation in 2 in.	Repeated Reversed Bend Number
		in.	in.		lb/sq.in.	%	
9	100		0.1045	2.30			
20	150		.1027$_5$	2.26			
20	153		.0785	1.72			
2	12	0.0470	.1562$_5$	3.36	60 500	10.0	14
2	13		.1065	2.29			
3	28		.0885	1.88			
3	39		.1015	2.18			
13	149		.1140	2.45			
22	177		.2110	4.54	60 600	13.0	20
21	201		.1170	2.52			
21	205		.1065	2.29			
31	209		.0910	1.96			
2	21	0.0480	.1065	2.24			
2	23		.1085	2.24			
3	25		.0885	1.82			
3	36		.1330	2.80			
6	64		.1170	2.45			
8	78		.1015	2.14			
13	141		.1405	2.96			
20	151		.0995	2.09			
19	161		.0747$_5$	1.57*			
14	163		.1350	2.83			
14	164		.1170	2.46			
24	173		.0747$_5$	1.57	56 700	13.5	31
22	179		.0830	1.75	54 500	9.0	32
23	189		.1005	2.11			
22	196		.1015	2.14			
13	118	0.0490	.1020	2.10			
13	147		.1027$_5$	2.12			
19	159A		.0747$_5$	1.54	62 200	14.5	25
19	160		.0910	1.88			
22	175		.0995	2.05			
23	176		.0980	2.02			
3	24	0.0500	.0865	1.75			
5	61		.1485	3.00	60 400	11.0	
9	98		.1065	2.15			
12	129		.0830	1.68			
20	152	0.0510	.1170	2.33			
8	93	0.0520	.1330	2.58			
22	178	0.0540	.1005	1.88			
1	1	0.0550	.1085	1.95			
1	3		.1140	2.09			
1	10		.1085	1.95			
2	17		.1140	2.02			
2	19		.0895	1.83			
5	54		.1020	1.87			

* Bent too far.

5. Table 1 (Cont'd)

Piece Number	Specimen Number	Thickness T	Radius R	$\frac{R}{T}$	Tensile Strength	Elongation in 2 in.	Repeated Reversed Bend
		in.	in.		lb/sq.in.	%	Number
5	55		0.1065	1.95			
5	59		.1405	2.58			
5	60		.1170	2.15			
8	92		.1405	2.58			
13	140		.1795	3.29			
13	148		.1330	2.44			
14	163		.0940	1.72			
22	197		.1565	2.87			
2	11	0.0560	.1330	2.40			
2	20		.1485	2.68			
3	27		.1170	2.11			
3	43		.0900	1.62			
5	56A		.1405	2.53			
5	58		.1565	2.82			
8	87		.1250	2.25			
8	88		.1875	3.38			
9	104		.....*	3.60	57 800	8.0	22
9	108		.1170	2.11			
9	109		.0865	1.56			
13	145		.0910	1.64	59 500**	7.0	17
22	174		.1140	2.05			
22	198		.1140	2.05			
22	199		.1045	1.88			
21	202		.1640	2.96			
21	203		.1405	2.53			
21	204		.0995	1.79			
1	4	0.0570	.1250	2.21			
1	7		.1005	1.78			
5	56		.1015	1.79	60 800	12.5	42
5	57		.1250	2.21			
8	79		.1027[5]	1.82			
9	99		.1170	2.07			
9	107		.1250	2.21			
13	144		.1330	2.35			
13	146		.1065	1.88			
2	22	0.0580	.1170	2.04			
8	90		.1640	2.85	59 900	9.0	39
13	142		.1640	2.85			
3	49	0.0620	.1015	1.65			
3	28	0.0630	.1250	2.00			
8	85		.1170	1.87			
9	103		.1170	1.87			
1	2	0.0640	.1405	2.21			
2	15		.1170	1.84			
2	18		.1330	2.10			

* Accidentally broken.

** Out of gage marks.

6. Table 1 (Cont'd)

Piece Number	Specimen Number	Thickness T	Radius R	$\frac{R}{T}$	Tensile Strength	Elongation in 2 in.	Repeated Reversed Bend Number
		in.	in.		lb/sq.in.	%	
3	41		0.1405	2.21			
2	16	0.0650	.1330	2.08			
8	86		.1250	1.94			
3	51	0.0820	.1330	1.62			
10	212	0.0480	.0848	1.77			
10	213	0.0480	.0805	1.68			
16	214	0.0500	.1172	2.34			
16	215	0.0380	.1172	3.08			
16	216	0.0500	.0760	1.52			
16	217	0.0460	.0848	1.84			
16	218	0.0480	.0830	1.73			
16	219	0.0650	.1250	1.92			

As can be seen from the figure, the uncorroded comparison material gives with very few (3) exceptions lower $\frac{R}{t}$ $\left(\frac{\text{Radius of curvature at first break}}{\text{thickness}}\right)$ values than any of the *Shenandoah* specimens and the $\frac{R}{t}$ values of the *Shenandoah* specimens fluctuate widely.* The specimens marked "Huge" broke completely at the appearance of the first crack. In these cases, any close measurement of $\frac{R}{t}$ was impossible, but in each case an estimate gave it a value lower than 7.

Specimens showing large and small values of $\frac{R}{t}$ were examined micrographically (see attached metallurgical report). In all cases of large $\frac{R}{t}$ values, marked intercrystalline corrosion was found, accompanied by the characteristic "crazing" of the surface in the portion of least radius of curvature. On the other hand, in all cases of low $\frac{R}{t}$ values, little or no intercrystalline corrosion was found. A large $\frac{R}{t}$ value in the bend test seems, therefore, to be a certain indication of the existence of intercrystalline corrosion at the bent portion of the specimen.

In order to determine whether there was selective corrosion of the various samples, Figures 6 and 7 were plotted. Figure 6 shows the $\frac{R}{t}$ values for specimens from samples Nos. 3, 8 and 12 indicated by different symbols. These samples were chosen because

* See Bureau of Standards Supplementary Report, page 628.

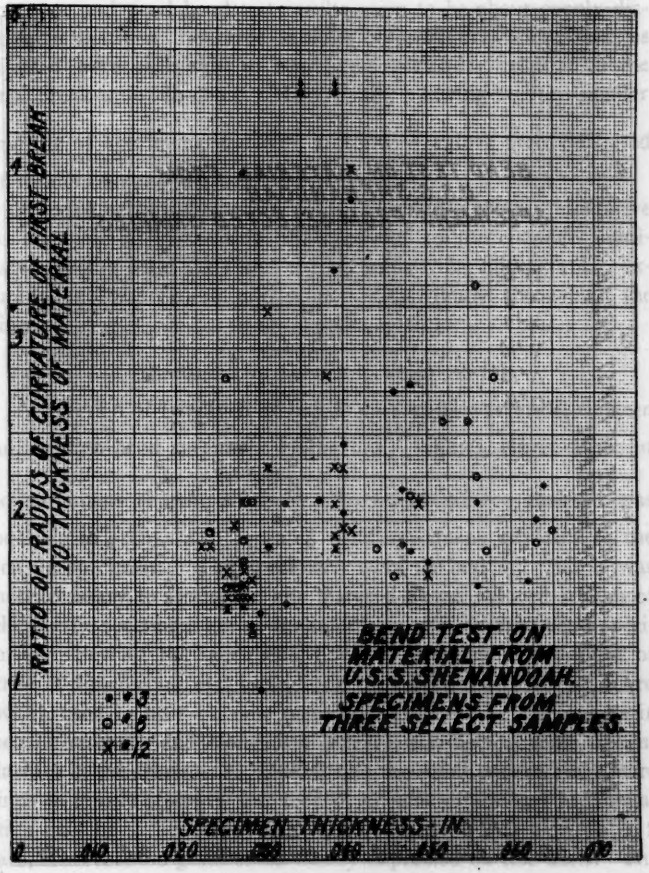


FIGURE 6.—BEND TESTS.

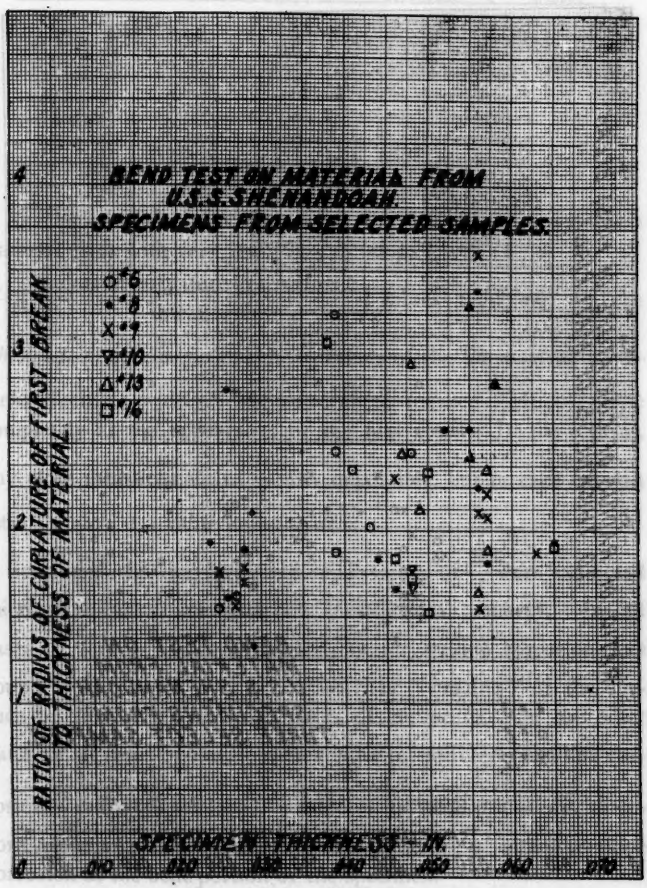


FIGURE 7.—BEND TESTS OF SPECIMENS SAID TO BEAR RELATION TO BREAK UP OF SHIP.

twenty or more bend specimens were obtained from each of them, and consequently the errors due to a small number of samples would be smaller for these than for any others. As can be seen, the poor and good bend test results seem to be about evenly distributed between the three samples. In Figure 7 the samples selected were each said to bear some particular relation to the final break-up of the ship. Here, again, allowing for the smaller number of specimens, the high and low $\frac{R}{t}$ values are distributed at random among the different samples.

It seems certain that so far as these samples are representative of the material in the ship, the corrosion of the channels (indicated by the bend tests and the accompanying micrographic examination) was general and randomly distributed throughout the ship.

TENSILE AND REPEATED BEND TESTS.

A selection of twenty-four tensile specimens was made, thirteen adjacent to bend specimens giving low $\frac{R}{t}$ values, eleven adjacent to specimens showing high $\frac{R}{t}$ values. In sixteen cases it was possible to secure also repeated bend specimens adjacent to the tensile specimens. The specimens were so laid out that it was not necessary to straighten any material in the reduced section of the tensile specimen, nor at the middle of the repeated bend specimens. Few of the specimens, however, were wholly straight or true in dimensions, and in addition they were not of the standard width used in tests on new material. Part of the low results in tensile strength and elongation may be ascribed to these irregularities in the specimens. One or two thousand pounds per square inch in tensile strength and 1 or 2 per cent differences in elongation may be so accounted for. The variations shown in Table 2 are, however, too great to be wholly explained on this basis. The variation in tensile strength from 45,200 to 63,200 pounds per square inch and from 5.0 per cent to 17.5 per cent elongation in two inches represent real differences in the material. In all cases the specimens showing low elongation showed evident crazing of

TABLE 2

TENSILE AND REPEATED BEND TESTS OF DURALUMIN FROM SHENANDOAH

Sample No.	Specimen No.	Thickness	Yield Point	Tensile Strength	Elongation in 2 in.	Reduction of area	No. of bends	Remarks regarding test
		in.	lb/in^2	lb/in^2	per cent	per cent		
2	12	0.0455	37 500	60 500	10.0	11.9	14	
3	33	.0288	42 400	60 400	9.5	11.2	—	
3	37	.0392	44 800	63 200	12.0	12.7	20	
3	45	.0284	45 700	57 800	6.0	11.4	—	Broke outside gage marks
3	46	.0260	41 500	61 400	17.5	17.7	—	
5	56	.0549	37 900	60 800	12.5	16.8	42	
5	61	.0466	40 400	60 400	11.0	10.9	—	
7	72	.0304	44 200	61 300	12.0	10.5	49	
8	90	.0544	37 900	59 900	9.0	9.6	29	
8	91	.0275	41 500	59 600	12.5	14.1	—	
9	104	.0537	41 000	57 800	8.0	11.9	22	
12	112	.0312	42 300	55 800	10.5	14.1	86	Out of gage marks
12	122	.0386	35 200	54 400	15.5	15.5	58	
12	124	.0400	39 200	60 800	12.0	16.1	—	
12	136	.0250	40 900	59 300	16.0	18.5	—	
13	145	.0538	39 400	59 500	7.0	11.2	17	Broke outside gage marks.
19	159A	.0479	35 100	62 200	14.5	14.2	25	
24	167	.0384	38 900	58 300	12.5	13.5	51	
24	173	.0468	32 900	56 700	13.5	15.8	31	
22	177	.0474	41 300	60 800	13.0	15.2	20	
22	179	.0470	33 600	54 500	9.0	14.5	32	
23	187	.0401	39 400	54 800	7.5	13.0	43	
23	188	.0415	35 700	53 500	5.0	5.9	11	
23	191	.0399	39 100	60 000	13.0	15.5	—	
18	A6	.0469	39 100	45 200	14.0	13.2	17	Badly corroded
18	A13	.0250	—	54 500	9.5	8.9	30	Eccentrically loaded
18	A33	.0462	32 000	54 200	10.0	10.8	—	
18	A48	.0248	37 100	55 300	14.0	17.8	59	
18	A51	.0463	44 100	58 700	12.0	14.7	16	

the surface, and on micrographic examination revealed intercrystalline corrosion (see micrographic report), while those showing high elongation showed little intercrystalline corrosion.

The repeated bent test was the same as used in the previous work on duralumin tested for fatigue, except that one-half inch (instead of one inch) was the greatest width which could be obtained. The specimen was stretched to 2000 pounds per square inch tensile stress, and under this tension bent backwards and forwards around a mandrel fifteen times its thickness until failure occurred. The results of these tests are also given in Table 2. No mandrel small enough for specimen No. 112 (0.30 inch thick) was available. It was therefore bent around the mandrel corresponding to a thickness of 0.040 inch thickness. This, presumably, accounts for the exceptionally high (86) number of bends obtained from this specimen. The range from the low value of eleven bends to the high value of 59 bends again shows the variability of the material. The specimens with low number of bends showed the typical patchy crazing of the surface, and on micrographic analysis (see metallographic report) revealed intercrystalline corrosion, while the specimens with a high number of bends showed little intercrystalline corrosion.

The evidence is conclusive that in all these specimens high $\frac{R}{t}$ value, low elongation, and small number of bends, are all due to the patches of intercrystalline corrosion on the surface of the specimen.

For comparison the results of the tensile and repeated bend tests are repeated in Table 1, opposite the adjacent bend specimens. It is evident that no apparent relation exists between the results of these three types of tests on specimens cut from adjacent parts of the same piece of channel. This is shown more clearly in Figure 8, where the $\frac{R}{t}$ ratios from the bend tests are plotted as abscissae and the tensile strengths and elongations from the tensile tests as ordinates. This lack of relation between the results of the tests is explained by the patchy nature of the corrosion. It is a matter of chance whether a relatively large corroded patch falls in the area tested in each case.

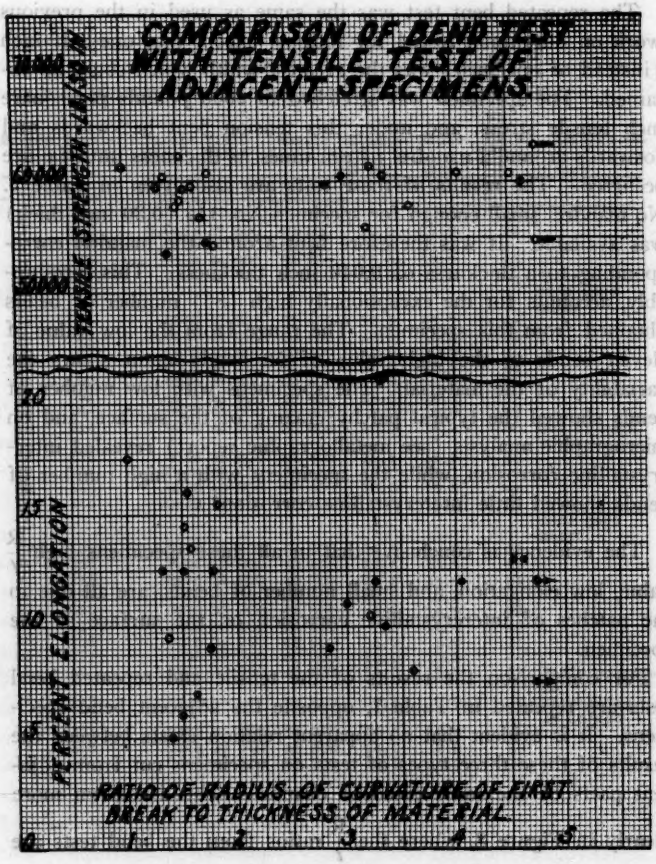


FIGURE 8.—COMPARISON OF BEND TEST WITH TENSILE TEST.

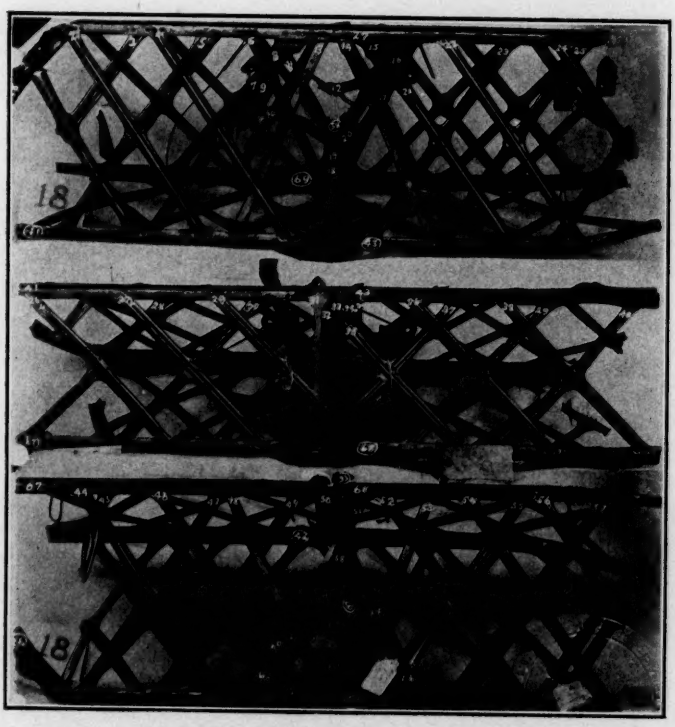


FIGURE 9.—SPECIMEN MARKED "BRITTLE."

SPECIAL EXAMINATION OF SAMPLE NO. 18 (MARKED "BRITTLE")

This sample was photographed in three positions and so far as possible each individual piece was numbered (see Fig. 9). The sample was then dismembered by drilling out the rivets without deforming the pieces and each piece carefully examined. No special evidences of corrosion were found. Tensile specimens (A6, A13, A33, A48, and A51, Table 2) were cut from five of the channels and adjacent repeated bend specimens from four. The tests results are also given in Table 2. Except for the one unusually low tensile strength (A6, Table 2), there is no indication that the material of these channels is on the average any more corroded or brittle than those from the other samples.

Finally the lattices were mounted in Woods Metal grips and given a rough tensile test. No attempt was made to secure axial pulling or freedom from bending, as the object of the test was merely to stretch the lattices sufficiently to bring out any intercrystalline corrosion and make possible a rapid and fairly complete survey. As the stretching had to be done in a testing machine, it was thought worth while to record the results obtained. These are given in Table 3. As can be seen, nearly half the specimens broke at or near the grips. Many of the others were torn rather than being stretched in pure tension, so that the results are wholly erratic. The pulling, however, served the purpose of bringing out the presence of intercrystalline corrosion to an extent not previously found in lattice specimens. This may be seen from the attached metallurgical report. The tensile test results also serve the purpose of showing that there was no dangerously weak lattice material in this sample. Taking the tensile test results at their face value, the lowest tensile strength recorded was over ten times as great as the lattice stresses found in the girder tests. Neither the tensile test results nor the presence of intercrystalline corrosion was found to bear any definite relation to the position of the specimen in the sample. Whether the lattice corrosion was greater in this sample labeled "brittle" than in the other samples cannot be stated, since there has not been time to explore the other samples systematically.

TABLE 3

TENSILE TESTS ON GIRDER LATTICES FROM SAMPLE NO. 18 FROM SHENANDOAH*

Specimen No.	Area	Ultimate Strength	Elongation in 2 in.	Remarks Regarding Test
	in.^{2}	lb/in^2	per cent	
1				Started to pull out at 1470 lb. load.
2	0.0268	59 000	10.0	
2	.0264	62 900	8.0	
3	.0258	41 500	2.0	Broke in grip.
4	.0252	58 800	6.0	
4	.0255	59 200	7.0	
5	.0269	41 300	1.0	Broke at edge of grip.
5	.0266	57 800	8.0	
7	.0261	58 200	5.0	
21	.0278	55 800	6.0	
22	.0267	44 200	4.0	Broke at edge of grip.
22	.0263	54 400	8.0	
23	.0263	48 700	2.0	
23	.0259	47 900	2.0	
24	.0257		2.0	Pulled out at 1120 load.
25	.0251	55 400	10.0	
25	.0255	59 600	4.0	
26	.0244	44 700	3.0	Broke in grip.
28	.0276	60 200	6.0	
28	.0288	48 900	9.0	Pulled out partly one side-eccentric.
29	.0239	40 200	2.0	Broke at edge of grip.
29A	.0229	28 800	0	Broke about 1/16" from one grip.
29A	.0235	40 400	3.0	
35	.0235	45 900	1.0	Broke in grip.
36	.0231	53 700	6.0	Broke in grip.
36	.0240	49 800	4.0	Broke in grip.
37	.0241	60 600	7.0	
37	.0242	41 300	3.0	Broke at edge of grip.
38	.0265	43 400	2.0	Broke in grip.
38	.0270	50 800	4.0	
39	.0241	57 300	8.0	
39	.0237	57 000	6.0	
40	.0248	45 500	2.0	
40	.0248	49 800	3.0	
44	.0244	57 400	4.0	
46	.0255	29 000	1.0	
46	.0255	37 300	1.0	Broke in grip.
47				Pulled out at 970 lb. load.
47				Pulled out at 1450 lb. load.
49	.0272	36 400	3.0	Broke at edge of grip.
49	.0267	49 400	1.0	Broke at edge of grip.
52	.0263	53 200	6.0	Broke at edge of grip.
52	.0255	53 800	5.0	
54	.0256	49 200	3.0	Broke at edge of grip.
54	.0261	49 000	5.0	
55	.0284	53 500	4.0	Broke in grip at rivet hole.
55	.0276	57 200	5.0	Broke at edge of arm.
56	.0251	54 600	5.0	
57	.0285	43 000	1.0	
57	.0284	60 200	6.0	Broke in grip.

* No attempt was made to secure axial pulling or freedom from bending, as the object of the test was merely to stretch the lattice sufficiently to bring out any intercrystalline corrosion and make possible a rapid and fairly complete survey. As the stretching had to be done in a testing machine, it was thought worth while to record the results obtained. As can be seen, nearly half of the specimens broke at or near the grips. Many of the others were torn rather than being stretched in pure tension, so that the results are wholly erratic.

MODULUS TESTS.

The importance of the modulus of elasticity in determining the strength of the girders made a special examination of this point desirable. For an accurate modulus determination accurately dimensioned specimens are necessary. It was impossible to machine such specimens from the material available. However, with care it was possible to obtain specimens of definitely corroded material of four different thicknesses satisfactory for a cantilever test to determine any possible difference between the modulus of the corroded surface in tension and compression. Nearly straight specimens approximately six inches long were secured, and one corroded surface machined off to a depth of over 0.005 inch, leaving the other corroded surface intact. These were mounted in a symmetrical clamp as cantilever beams, and tested under equal loads, first with the machined side *down* (corroded surface in tension), and second with the machined side up (corroded surface in compression). The deflections were measured by a cathetometer to thousandths of a centimeter. The results are given in Table 4. These results fluctuate due to unavoidable errors, but the average deflection "machined side up" differs by only 1¼ per cent from the average deflection "machined side down," showing that within experimental errors the modulus of the corroded material is the same in tension as in compression.

Finally a very satisfactory specimen was obtained from near a bend specimen which showed considerable corrosion (No. 92 $\frac{R}{t}=2.58$). This was, so far as examination showed, uniform along its length, but the curvature of the section made an accurate micrometric determination of its area impossible. It was therefore machined accurately to length (8.67 inches) and its volume (3.889 cubic millimeters) determined by the Volumetric Section of the Bureau of Standards. This gave as accurate a determination of the sectional area as could be made. The specimen was then mounted in clamps six inches apart and two optical strain gauges (two inches gauge length) mounted on opposite sides. These gauges, which show elongations as small as one-millionth of an inch per inch, were calibrated immediately beforehand by

TABLE 4

MODULUS TESTS ON DURALUMIN FROM SHENANDOAH

Cantilever Test

Load	Machined Side Down			Machined Side Up		
	Reading	Deflection	Deflection per unit load	Reading	Deflection	Deflection per unit load
grams	cm.	cm.		cm.	cm.	
			Specimen Number 62 ($\frac{E}{\sigma}$ = 2.02)			
0	76.928			76.746		
25	76.716	.210	.210	76.534	.212	.212
0	76.924			76.746		
				76.738		
50	76.502	.420	.210	76.320	.422	.211
0	76.920			76.748		
75	76.294	.631	.210	76.114	.634	.211
0	76.930			76.750		
100	76.090	.842	.210	75.900	.848	212
0	76.934			76.746		
			Specimen Number 69 ($\frac{E}{\sigma}$ = 1.55)			
0	76.648			76.798		
15	76.424	.223	.222	76.578	.216	.216
0	76.646			76.792		
30	76.210	.435	.218	76.356	.435	.217
0	76.644			76.794		
45	75.998	.646	.216	76.146	.647	.216
0	76.644			76.792		
60	75.776	.867	.217	75.926	.865	.216
0	76.642			76.790		
			Specimen Number 121 ($\frac{E}{\sigma}$ = 1.56)			
0	75.664			76.286		
5	75.424	.240	.240	76.038	.247	.247
0	75.664			76.284		
10	75.190	.475	.238	75.796	.490	.245
0	75.666			76.288		
15	74.954	.712	.237	75.552	.734	.245
0	75.666			76.284		
20	74.728	.939	.235	75.312	.973	.244
0	75.664			76.286		
			Specimen Number 197 ($\frac{E}{\sigma}$ = 2.27)			
0	76.786			77.038		
30	76.604	.181	.181	76.854	.184	.184
0	76.784			77.038		
60	76.416	.366	.183	76.664	.374	.187
0	76.780			77.038		
90	76.236	.547	.182	76.483	.555	.185
0	76.786			77.036		
120	76.050	.733	.183	76.298	.739	.184
0	76.780			77.038		

means of an interferometer. In order to check the work a duplicate test was made on a specimen of uncorroded material from stock. The specimens were loaded with dead weights and the elongations recorded. The results are given in Table 5. The modulus determined for the corroded *Shenandoah* material aver-

TABLE 5

MODULUS DETERMINATION UPON SHENANDOAH MATERIAL FROM NUMBER 8 SAMPLE

(Near Specimen No. 92 - $\frac{E}{E_0}$ value of 2.58)

AND NEW MATERIAL ($\frac{E}{E_0}$ value 1.35)

Material	Load	Stress	Difference in Stress	Autocollimator Reading #1	Reading Number 1 corrected Factor 0.988	Autocollimator Reading #4	Reading Number 4 corrected Factor 0.964	Average Extension	Young's Modulus x 10^6
	lb.	lb/in^2	lb/in^2	0.0001 inches per inch					
Shenandoah	26.06	954		4.08	4.03	1.36	1.31		
"	66.06	2419	1465	5.04	4.98	3.21	3.09	1.385	10.7
"	26.06	954		4.08	4.03	1.36	1.31		
"	36.06	1318		4.54	4.49	1.99	1.92		
"	116.06	4252	2939	7.00	6.92	5.26	5.07	2.79	10.5
"	36.06	1318		4.52	4.47	2.01	1.94		
"	36.06	1318		4.46	4.41	2.04	1.97		
"	156.06	5720	4398	8.29	8.20	6.74	6.50	4.165	10.6
"	36.06	1318		4.44	4.39	2.04	1.97		
New	26.06	1040		3.94	3.90	2.25	2.17		
"	66.06	2640	1600	5.55	5.49	3.86	3.72	1.565	10.2
"	26.06	1040		3.95	3.91	2.26	2.18		
"	26.06	1040		3.95	3.91	2.26	2.18		
"	106.06	4240	3200	7.19	7.11	5.46	5.26	3.140	10.2
"	26.06	1040		3.96	3.92	2.26	2.18		
"	26.06	1040		3.96	3.92	2.25	2.17		
"	146.06	5840	4800	8.82	8.72	7.08	6.82	4.725	10.2
"	26.06	1040		3.96	3.92	2.25	2.17		

aged 10,600,000 pounds per square inch, while that for the new material averaged 10,200,000 pounds per square inch. The differences are within the experimental error.

Intercrystalline corrosion, therefore, to the extent found in these specimens does not appreciably alter the modulus of elasticity of the material.

FATIGUE TESTS.

The question of fatigue tests was carefully considered. Previous work has shown that conclusions drawn from short time high stress range tests cannot with certainty be extrapolated to

long time low stress range tests. In the previous investigation of corroded *Shenandoah* material * made by the Bureau of Standards, it was found that material from a scrap heap, more severely corroded† than any piece found in the wreck showed a lowering of the stress range which the material could endure for a given number of vibrations to from 60 to 80 per cent of that for uncorroded material. These tests were all at high stress ranges, 35,000 (± 17,500) to 50,000 (± 25,000) pounds per square inch, but one specimen was removed unbroken after fourteen million four hundred and thirty-one thousand alternations at a stress range of 27,200 (± 13,600) pounds per square inch.

The time required for low stress range tests is large. In the fatigue investigation of uncorroded material some specimens were subjected to continued alternations of stress for over a year. As a repetition of high stress range tests could only serve to confirm the results of the previous investigation it was felt more desirable to spend the time available for the investigation in determining the extent to which corrosion was present in the samples from the wreck, reserving specimens from the more severely corroded parts for fatigue tests. As time permitted, fatigue tests were started, but no results are at present available. As noted before, a thorough search of the breaks in the wreckage at Lakehurst and a repeated search through the samples sent to the Bureau of Standards, where especial attention was given to samples No. 14 and No. 17, said to have been subject to vibration, failed to show any sign of a fatigue break.

TENSILE TESTS ON SHEAR AND NETTING WIRES.

Standard tensile tests were made on nine shear and netting wires. Three of these were broken in the wreck and the others were adjacent to these. The results are given in Table 6. These wires had been severely over-strained in the wreck, which is sufficient to account for the irregularity of the elongations found and the frequency of breaks outside of the measured gauge length.

In spite of this the tensile strengths are uniformly high.

* Report on "Embrittlement of Duralumin," May 14, 1925.

† Bureau of Standards Report VIII-1/Tsm 42532 M.L. No. 5959, Feb. 2, 1925.

TABLE 6

TENSILE TEST OF STEEL WIRE FROM SHENANDOAH

Sample Marked	Specimen Number	Thickness	Gage Length	Yield Point	Tensile Strength	Elongation in 8 in.	Remarks
		in.	in.	lb/in²	lb/in²	per cent	
25	1	0.072	8.0	--- ---	251 000	2.0	Showed bend break
25	2	.072	8.0	143 000	239 000	1.4	
29	1	.1025	8.0	156 000	222 000	2.8	
29	2	.1025	8.0	131 000	222 000	2.9	
29	3	.1025	8.0	149 000	215 000		Broke outside gage marks.
29	4	.1025	8.0	151 000	226 000		Broke outside gage marks. Showed tensile break.
29	5	.1025	8.0	166 000	221 000	2.5	
29	6	.1025	8.0	152 000	223 000		Broke outside gage marks.
9	-	.113	8.0	171 000	215 000	1.4	Showed tensile break.

As these tests showed a uniformly high quality in the wires tested, no further tests were considered necessary.

It may be noted that the maximum design stress computed for any of these wires * was 126,000 pounds per square inch.

TESTS ON CONTROL SPROCKET CHAINS.

Special adapters were machined to fit over three links of the chain and two tests of the breaking strength made. Since the breaks were in the free length, the adapters were suitable. The two tests gave a breaking strength of 2025 and 2030 pounds, respectively, the failure being by shear through the rivets. The break in the wreck was by tearing out of the two eyes of a link.

The detailed report on the metallographic examination follows:

METALLOGRAPHIC EXAMINATION.

The aim of the metallographic examination which was intended as a supplement to the mechanical tests, was to show whether any corrosive attack had occurred and particularly to show whether such attack as may have occurred was intercrystalline in its nature.

* AER-M-13-MF; O-R-4—Design Memorandum No. 7, by C. P. Burgess, July, 1922, p. 22.

In previous examinations of various miscellaneous duralumin specimens, it had been found that corrosion of the material had often occurred as an intercrystalline penetration of the metal rather than as a more or less uniform corrosion of the surface. An intercrystalline corrosion attack is generally recognized as having a much more pronounced effect upon the properties of a metal than simple surface corrosion has. Intercrystalline corrosion usually results in the embrittlement of the metal which is attacked, the degree of embrittlement being dependent upon the severity of the attack.

A series of samples was taken from many of the larger specimens submitted. These are shown in the attached photographs, Figures 10 and 11, together with the assigned number of the material from which the samples were taken. These samples included both channel and lattices, and were chosen so as to include the fractured ends. It was assumed here that these fractures occurred in service, that is, at the time of the wreck of the *Shenandoah*. In each case they were cut into small lengths (1 inch or so) and these were assembled side by side like the pages in a book and held firmly together by means of small bolts. A section through each of the assembled specimens was then prepared by suitably grinding and polishing it in the well-known manner, generally approved for metallographic work. Each specimen examined, therefore, consisted of a number of pieces of channel or lattice material as the case might be sectioned longitudinally perpendicular to the surface of the material examined. Both sides of the sectioned sheet material were examined for evidence of corrosive attack, that is, both surfaces were examined. After the first examination, all the specimens were deeply ground off, repolished, and re-examined to see if the results of the first examination were confirmed.

In general the examination indicated superficial intercrystalline corrosion of most of the pieces examined. Specimen 18-1 (channel) was found to show more pronounced evidence of intercrystalline corrosion than any of the others examined. Even in this case, however, the depth of penetration was slight as compared with the total thickness of the material. Fig. 12 shows a series of micrographs representing a section through a channel of specimen

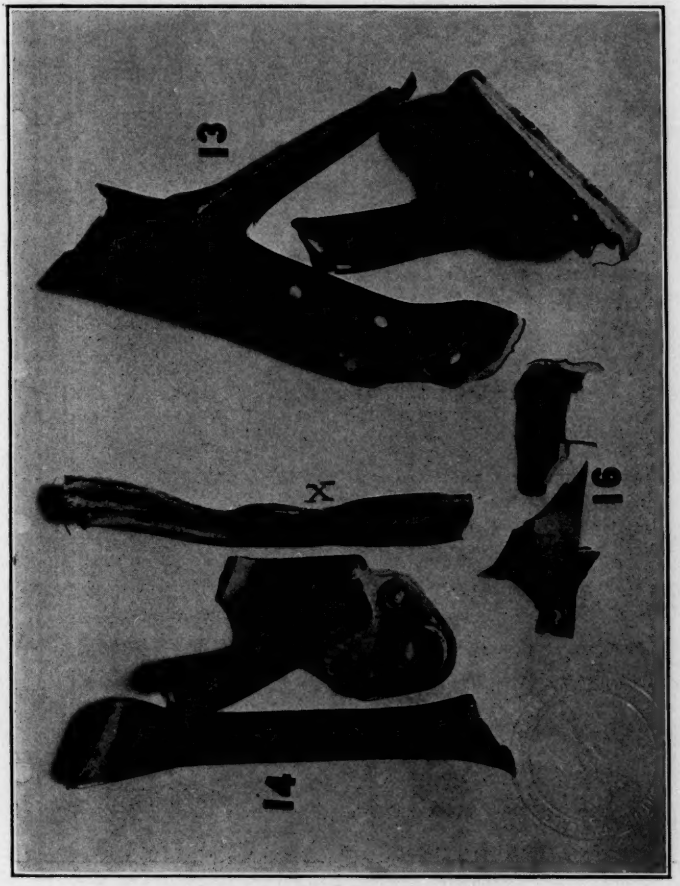


FIGURE 10.

Samples Cut from the Larger Specimens, Indicated by the Numbers, for General Microscopic Examination. The Marked Lines Indicate Service Fractures.

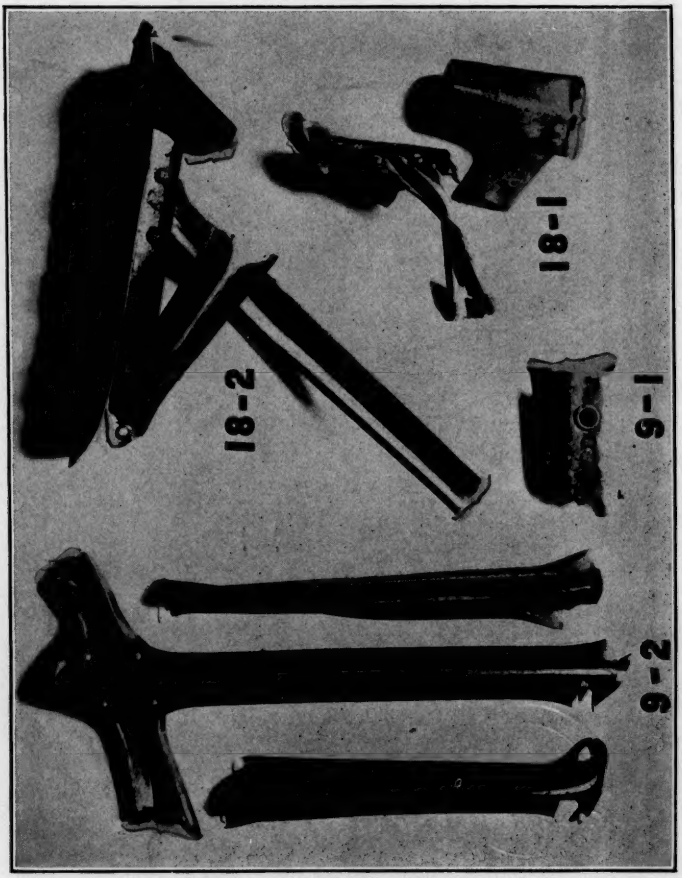


FIGURE 11.

Samples Cut from the Larger Specimens, Indicated by the Numbers, for General Microscopic Examination. The Marked Lines Indicate Service Fractures.

FIGURE 12.— SECTION THROUGH A CHANNEL OF SPECIMEN 18 — 1 × 250.

This was the one which was marked "brittle" by the officers who selected the material at the scene of the wreck. The section was etched with 10 per cent sodium hydroxide solution. The corrosive attack at this point had penetrated to a depth of approximately 0.002 inch (½ inch at a magnification is equivalent to 0.002 inch actual penetration). The branching network is characteristic of intercrystalline corrosion.

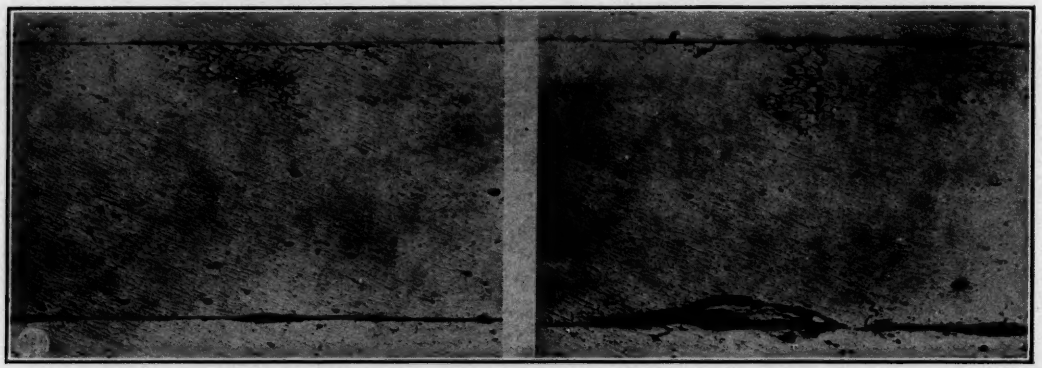


FIGURE 13.— LATTICE FROM SPECIMEN 14 × 100 UNETCHED.
The micrograph shows that the attack had penetrated in the spot shown .008 inch.

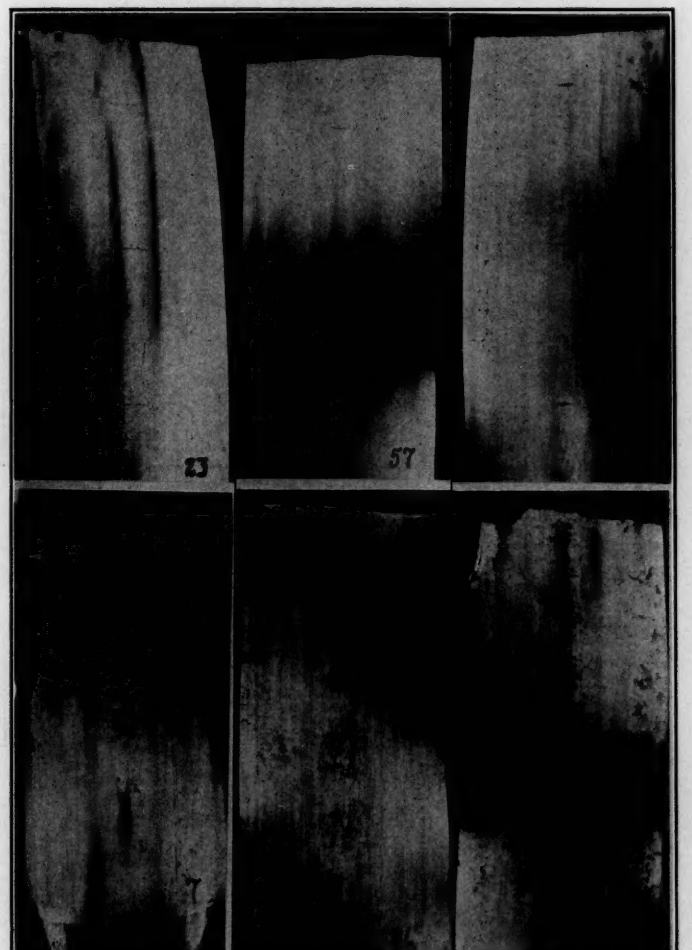


FIGURE 14.—ETCHED SURFACES OF TESTED LATTICE SAMPLES FROM SPECIMEN 18 ("BRITTLE"). × SLIGHTLY LARGER THAN NATURAL SIZE.

The specimens were rolled flat after completion of the tensile test. The sample number is indicated in each case. In samples 46 and 7 the "under" or concave side is shown; in all of the others, the "outer" or convex side of the lattice showed greater corrosion.

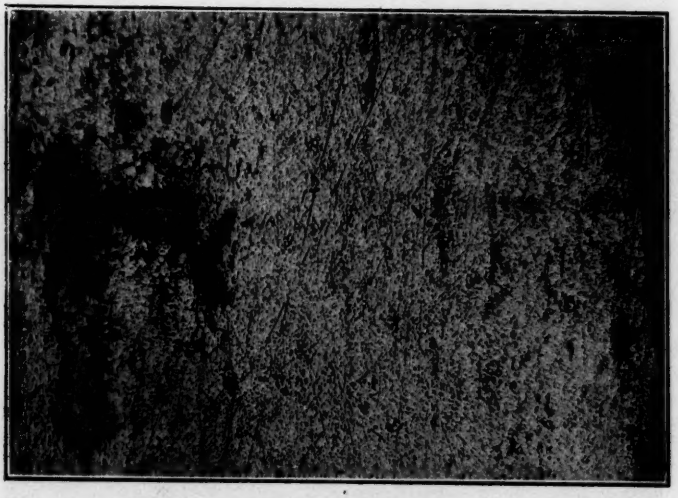


FIGURE 15.— MICROGRAPH OF LATTICE SAMPLE 49 (FROM FIGURE 14) × 250.

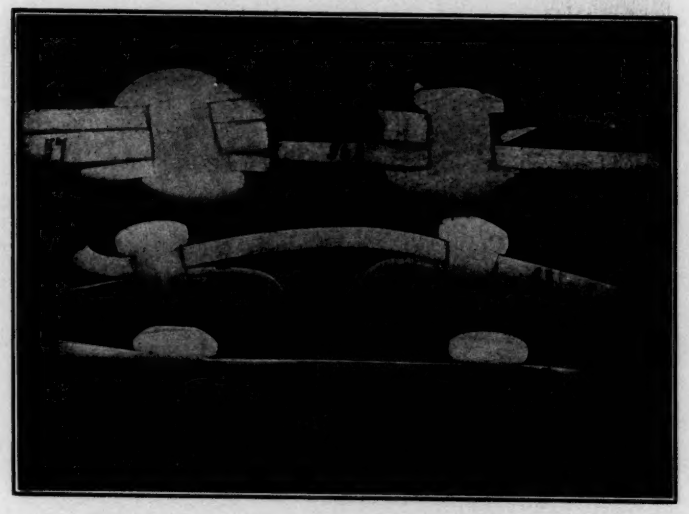


FIGURE 16.— DURALUMIN RIVETS SECTIONED, × 2.

18-1. The depth to which the corrosive attack had penetrated is very evident.

Specimen 18 is the one which was marked "brittle" when received at the Bureau of Standards for examination. The other channels showed slight traces only at isolated spots, much less than that shown in Figure 12.

From the results of previous examinations, the conclusion had been reached that the duralumin channels of the *Shenandoah* were very much more prone to corrosion of the intercrystalline type and that the lattices were practically exempt from such an attack. In general, the examination of the specimens covered by this report does not confirm entirely this conclusion. Evidence of intercrystalline corrosive attack was shown by a number of lattice bars. The most pronounced case was a lattice sample taken from specimen 14. This is indicated by X in the photograph of Figure 10. Evidence of corrosion on the surface was plainly visible to the eye and when sections were examined microscopically, it was found to be corroded in some spots in an intercrystalline manner to a depth of .008 inch. This is shown in Figure 13.

As part of the test of the material of specimen 18 (marked "brittle") — 49 lattice specimens were pulled (as explained under mechanical tests), 48 of the specimens were etched with dilute hydrofluoric acid and cleaned with dilute nitric acid and water. Some evidence of "crazing" was shown by most of the etched specimens. Of the 48 specimens examined 11 were classed on the basis of their appearance under a hand lens as showing a severe attack, 31 as showing a slight attack (that is, anything from a trace to a plainly visible attack) and 6 as unattacked. The samples varied as to the side showing the greater amount of corrosion, that is, some specimens showed more on the convex or "outer" side of the lattice, whereas in others the attack was more pronounced on the concave side.

Figure 14, which shows the appearance of six of the specimen, illustrates the wide variation of the attack found in the lattices examined. The specimens were rolled flat after the test was completed. The photographs show the flattened portion at the fractured end of the sample. In each case the upper side of the photograph represents the fracture.

Figure 15 shows a micrograph of a section of sample (Figure 14). It is evident from this that the corrosive attack is intercrystalline in nature. The depth of penetration shown here is .0005 inch.

RIVETS.

It was noticed in the visual examination of the materials submitted that some of the rivet heads showed what appeared to be evidence of corrosion. A number of rivets which showed this were sectioned and examined microscopically. Figure 16 shows the sectioned rivets.

The numbers are those assigned when the materials were cut up for mechanical tests. The examination showed some intercrystalline corrosion on the outer surface of the heads of the rivets, but none whatever was found on the shank or the inner side of the head where it was in contact with the sheet materials which were riveted together. Some of the rivets showed that the riveting pressure did not always give uniform heading; this resulted in a fold at the junction of shank and head which slightly lessened the effective area of the rivet shank (about 4.5 per cent in the specimen 56*a*). Figure 17, a section through the head of one of the rivets, shows the extent of the corrosion penetration here.

BEND SPECIMENS.

In Figure 18 is shown the appearance of the outer (convex) surface of a number of representative bend specimens after being tested. These show the network of fine surface cracks which formed on the specimens of low bending properties. The photographs are representative of specimens varying from those which could be bent double (No. 94) without fracturing to those which proved relatively brittle under the same test (No. 161). As soon as any cracks appeared on the surface as the bar was bent, the test was stopped.

The number assigned to each bend specimen is indicated. The surface of the metal was cleaned after bending, by etching it slightly with dilute hydrofluoric acid. By cleaning in this manner the cracks are widened and thus revealed much more plainly than

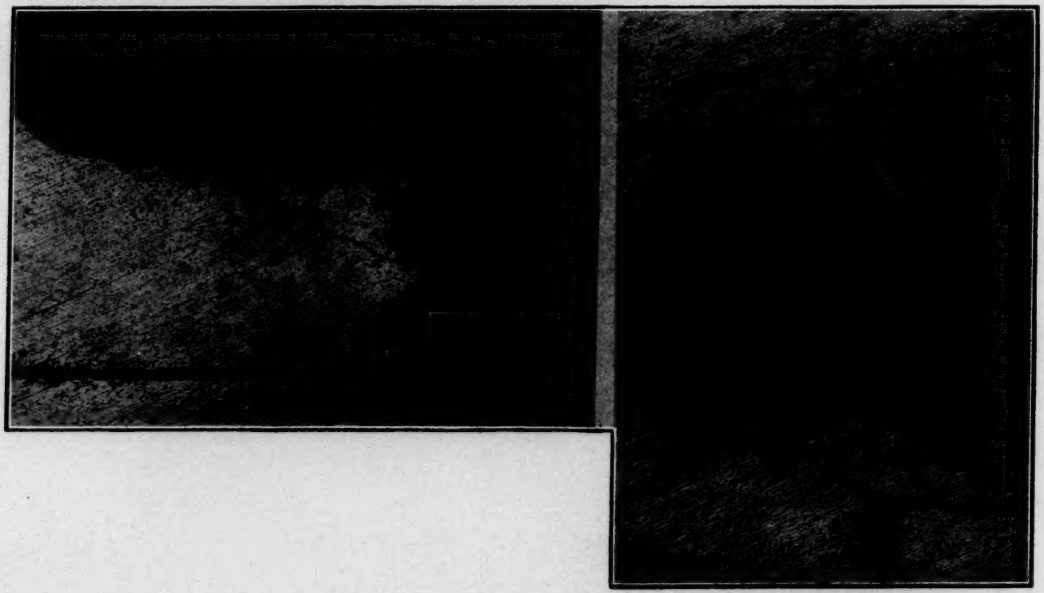


FIGURE 17.

(*a*) Section through the head of a rivet (specimen 55 of Figure 16) × 100, etched with 10 per cent sodium hydroxide. The corroded position is indicated in brackets.

(*b*) Section through a shank of a rivet, no intercrystalline corrosion attack was found on the shank or on the adjacent lattice.

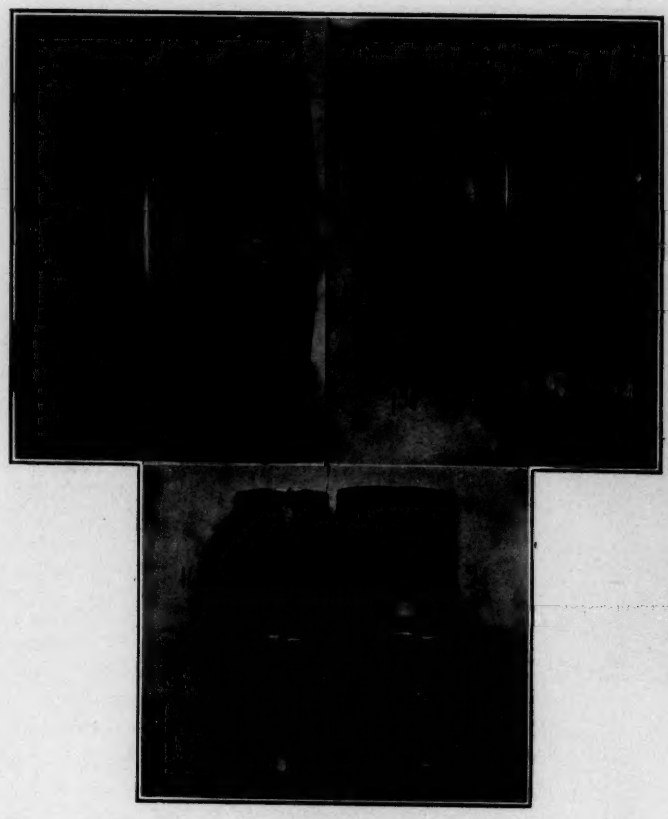


FIGURE 18.—PHOTOGRAPH, × 2, OF REPRESENTATIVE BEND SPECIMENS AFTER COMPLETION OF THE TEST.

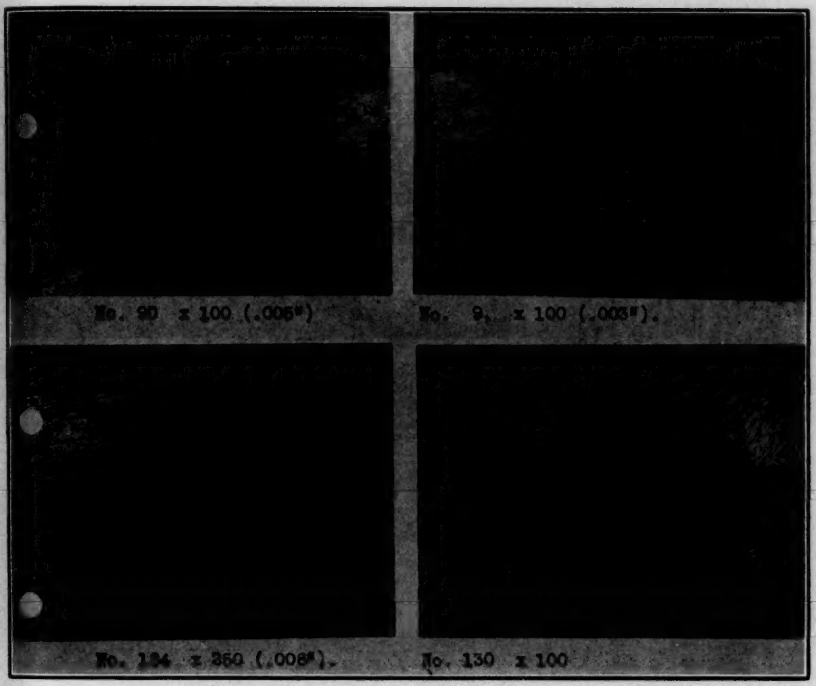
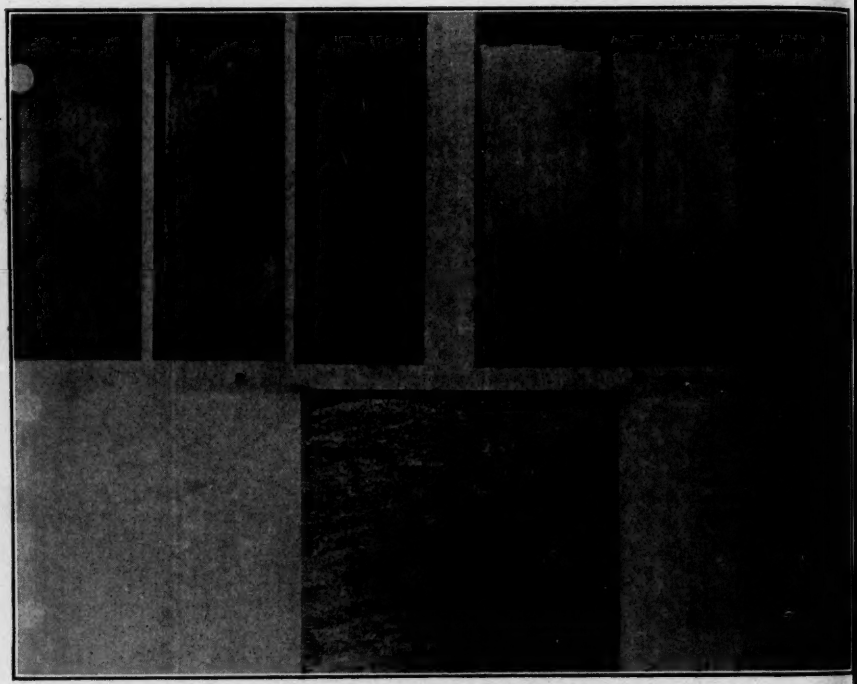


FIGURE 19.

Longitudinal sections of bend specimens showing the relation between fractures and intercrystalline corrosive attack of the duralumin at the surface. The approximate depth to which the attack had penetrated is indicated in each case. No. 130 bent without showing the surface network of cracks except in a narrow streak near the edge of the specimen. Only slight traces of intercrystalline attack were found in this specimen. In each case the outside or convex side of the "bend specimen" corresponds to the upper edge of the micrograph.



Specimen 37; × 250, depth of layer showing intercrystalline corrosive attack, .008 inch. Etched with 10 per cent sodium hydroxide solution.

FIGURE 20.— REPEATED-BEND SPECIMENS.

a shows a group of specimens (37, 188, 179) which developed the surface crazing during the test, those in *b* did not (13A, 72, 122); + 2. The specimens were cleaved by etching with dilute hydrofluoric acid followed by dilute nitric acid.

Micrograph *c* shows (× 250) a section of specimen 37. The relation between the cracks which develop the bend test and the intercrystalline corrosion attack is evident from the micrograph.

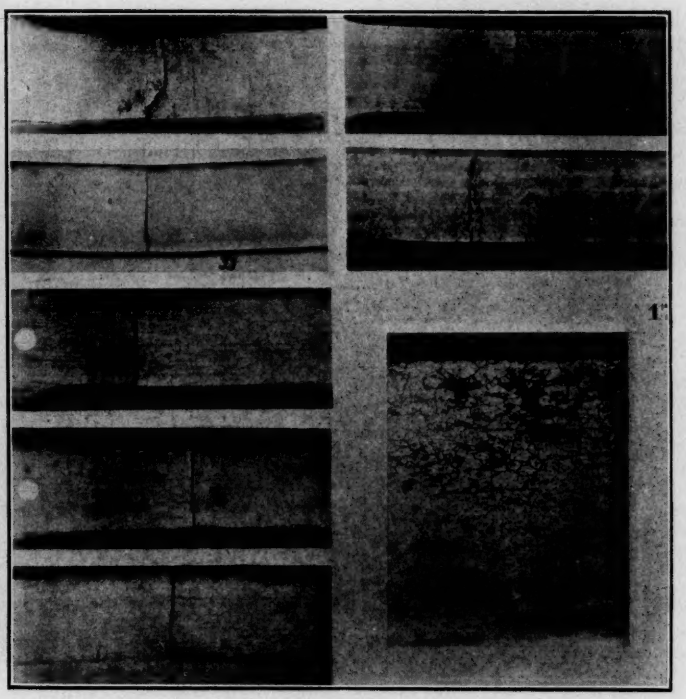


FIGURE 21.

Tensile specimens after testing, × 2. The specimens were cleaned with dilute hydrofluoric acid followed by dilute nitric acid.

The micrograph shows a section of specimen 188. The number and size of the surface cracks which appear as the specimen is stressed is a good indication of the severity of the corrosive attack of the metal. In this case, the attack had penetrated to a depth of approximately .007 inch. The specimen was etched with a 10 per cent aqueous solution of sodium hydroxide.

immediately after testing. This is also true of all the other test specimens examined.

In the microscopic examination of these specimens, each bar was ground down on the edge until the section passed through a patch of the network of cracks, that is, in those specimens which showed such cracks. The aim of the examination was to show the dependence of the surface cracks, which open up upon bending, on the intercrystalline corrosive attack of the metal as indicated by the branching network of fissures penetrating into the metal for a slight distance from the surface.

All of the "bend-test" specimens which showed the surface cracks at large curvatures were found upon microscropic examination to show the intercrystalline corrosion attack. Specimens which bent to small radii before developing surface cracks showed less widening of the cracks on etching and little or no evidence of intercrystalline corrosion. This is shown in the following micrographs which illustrate the relation between the surface cracks indicating the beginning of fracturing by bending and the intercrystalline corrosion attack of the surface metal. Nos. 184 and 130 were etched with 10 per cent sodium hydroxide solution, the others were unetched.

REPEATED-BEND SPECIMENS.

The aim of the examination was the same as that of the previous one, that is, to show that the "crazing" which was found on some of the specimens close to the fracture at the completion of the test was to be ascribed to the condition of the metal arising from intercrystalline corrosion attack. The accompanying photographs (Figure 20) show the fractured ends of three of the tested specimens which showed the network of cracks and three which did not. In the microscopic examination of sections of these specimens, it was found (as illustrated by the micrograph) that those specimens which developed the surface network showed evidence of intercrystalline corrosion attack.

TENSILE SPECIMENS.

A number of tensile test bars are shown in Figure 21. These were cleaned after being tested by etching in dilute hydrofluoric

acid and then in dilute nitric acid. The characteristic crazing which appears under tension in duralumin which has been subjected to an intercrystalline corrosion attack are shown. The "spotty" or "patchy" character of this type of corrosive attack is well illustrated by these specimens. The attack may vary in different specimens from a few isolated "check cracks" as shown in specimen 33 to a "crazing" which almost completely covers the surface (specimen 188). The fracture of the tested bar may be localized by a corrosion "patch" as illustrated by specimens 179 and A6.

SUMMARY OF METALLOGRAPHIC EXAMINATION.

The metallographic examination showed a general patchy intercrystalline corrosion of the channels and rivet heads. Ordinarily, the corroded patches are isolated from each other and cover only a small portion of the area. This corrosion may be present to a marked degree with little or no surface evidence and with no evidence of soiling of the material by corrosive liquids. This corrosion was not found under the heads of rivets nor in the rivet shanks. Evidence of intercrystalline corrosion was also found in some of the lattices to a considerable extent. One lattice specimen showing marked visible evidence of corrosion revealed intercrystalline corrosion on microscopic examination. Most of the lattice samples from specimen 18 showed surface evidence of intercrystalline corrosion to some extent. The amount of corrosion varied rather widely, however, in the different lattices.

The deepest spot of corrosion found in any of the material extended about .008 inch below the surface, but in the greater number the penetration was not more than .002 inch.

The characteristic "crazing" of the surface found in specimens which cracked in the bend test at large radii of curvature or which appeared in the tensile specimens with relatively low elongation was in every case due to a patch of intercrystalline corrosion at the point where the crazing appeared. This surface crazing may, therefore, be a reliable indication of intercrystalline corrosion in duralumin.

The microscopic examination gives no definite information as to how much the properties of the metal have been changed. This is shown by the results of the mechanical tests.

GEORGE K. BURGESS, *Director.*

Washington, D. C.
Nov. 19, 1925.

Dr. L. B. Tuckerman is an engineer physicist who has been employed by the Bureau of Standards since 1918. The report of the Bureau quoted above was made by him. After reading parts of the report, Dr. Tuckerman testified as follows:

TESTIMONY OF DR. TUCKERMAN.

Special examination was made of certain pieces which were said to bear certain definite relations to the break of the ship, and in these the same thing was found. In any one specimen you would find both relatively deep and relatively light spots of corrosion, and so far as anyone could see they were uniformly distributed throughout all the material investigated.

The same thing is repeated in the tensile bend tests, and then specimen No. 18 was most carefully worked out and tests made very thoroughly and no corrosion found, no worse in this than in the others, and no better, and no indication that it was in any way associated with any part of the structure. It is a general corrosion, patchy over the surface, small patches and large patches, and then the modulus tests were made and the figures are given here showing that there was no difference between tension and compression, and the figures showing what they are, and then the fatigue tests are mentioned and the shear and netting wire tests and the micrographic analyses.

There is one point that is not mentioned in the summary which I might call attention to. That investigation was made of the rivets, and while corrosion was found on the rivets, the heads of the rivets, it was never found in the shank of the rivets or under the rivets. That is in the metallurgical report, but in our hurry it was omitted from the summary, but this is a complete description of the findings in the microscopic analyses.

Q. Doctor, how far does the minute inspection of the surface of a sheet of duralumin reveal a state of corrosion? A. I wish I knew; I started looking this over and I inspected the surface very carefully, and I thought I found very, very few marks. The more I inspected and the more micrographic analyses I saw submitted, the more I thought that if a man has trained himself to do it that he could find it by visual inspection. Very minute discolorations of the surface I have found in looking over the material, and then adjacent to that I will find one with a little bit worse discoloration, and then one with slight worse discoloration, and then I will find one with that same type of a discoloration with a little bit of gelatinous aluminum oxide, which is characteristic of minor corrosion. We haven't had time to follow that out further, but such few tests as I have been able to make, make me think that certain types of discoloration of the varnish and certain little pin-point discolorations of the surface of the material are always associated, though I would not want to make any such statement. I think, but I do not know.

Q. What are the visible signs on the surface which would lead one to suspect deterioration? A. Well, they are minute, just that. Of course, if you see any efflorescense of a white powdery material, or aluminum oxide, you know it is corroded. Or, if you see little gobs of gelatinous material which looks like drops of hardened honey you know it is corroded under those spots. But if you do not see the white spots, these little gelatinous drops, unless the statement that I made before is verified, there is very little show in the surface of the material that your intercrystalline corrosion has taken place. I might mention that in the report. In the visual examination we hunted and marked those before making the tests visibly showing corrosion, and those which were markedly corroded, and so far as the tests showed they fell in among the other tests as distributed — that is, the visible evidence is no evidence of particularly severe or of particularly small corrosion.

Q. Is there any other form of deterioration besides intercrystalling corrosion to which you have referred in your previous answer, and if so, what is the influence of such corrosion on the properties of the material? A. There is another type of corrosion which was familiar to us before we commenced this study, and that is a pit-

ting corrosion. The corrosion of the intercrystalline type is a weakening of the connections between the aluminum crystallines in the materials. The other is an eating away of the surface into pits, a complete eating away of the surface of the material, forming little pits like on a badly rusted piece of iron. You scrape it off and you see that it is pitted in pits. That apparently does not lower the elongation of the material as much as the intercrystalline corrosion.

Q. Can intercrystalline decomposition always be anticipated by the condition or appearance of the surface? A. I do not know that I can answer that much more than I have. I should say no. I think this, that if you have a surface which has a bright and polished look, and you know that nobody has been rubbing that surface since the material was made, I would be pretty sure that there was no corrosion there, but if somebody had been rubbing the surface, or if cloth had been rubbed over it, I would not say that it could be seen.

Q. How does the resistance to vibration of duralumin compare with that of steel? A. That depends upon what steel you are talking about. For not too highly tempered steels, that is, those that are not hardened more than a certain amount, there seems to be a fairly consistent relation, about 50 per cent of the tensile strength is the stress at which the material will endure an indefinite number of alternations of stress without breaking. That is not absolutely determined, but that seems to be indicated by a large number of investigations. In duralumin so far no series of tests has definitely determined the existence of such an endurance limit. Our tests, for instance, indicated that the duralumin of which the *Shenandoah* was made would endure, say a billion alternations of stress at about a range of stress of 24,000 pounds per square inch, but the curves that we drew show, as far as our tests went, which were about 200 million alternations of stress, that the curve was still going down, not indicating that it had reached a horizontal portion, so that it would continue indefinitely to stand stresses of that kind. I should say that anywhere from 24,000 to somewhere around 35,000 pounds per square inch range of stress — that is to say, from minus or plus 12,000 pounds per square inch up to minus or plus 17,500 pounds per square inch — would be the

stress range which good duralumin would be expected to endure one billion alternations of stress — in sheet form — and that coincides pretty fairly well with the bar experiments in the rotating beam.

Q. Doctor, are you able to make the reports and other documents relating to previous fatigue tests made by the Bureau of Standards available to the court? A. Those will be found in the files of the Bureau of Aeronautics, and I think most of those the Bureau of Standards has only its own personal file of, and while we could make copies and will be glad to make them, if you could get them elsewhere we would prefer that. All of these, of course, were reported to the Bureau of Aeronautics and to the National Advisory Committee for Aeronautics.

Q. Did you examine part of the structure at or near the suspensions or struts of the power cars? A. If by that you refer to those specimens numbered 14 and 17, I believe they were said to come from near the power cars. They were said to be subject to vibration, because they were there, and those particular specimens I examined most carefully and found no sign whatsoever of a fatigue break.

Q. To what extent in your opinion might the imbrittlement of the duralumin, as shown by test specimens from the wreck of the *Shenandoah*, influence the strength of the ship? A. It would not weaken the ship, in so far as static and aerodynamic loads are concerned. If any portion of the ship was subject to excessive, or very large vibratory stresses, say of the order of 10,000 to 15,000 pounds per square inch, a range of stress of 20,000 to 30,000 pounds per square inch, it would weaken the ship materially.

Q. Summing up, I understand you to say that intercrystalline corrosion lowers the ultimate tensile strength of the material from about 60,000 pounds per square inch to not less than 45,000 pounds per square inch, while the elongation under a tensile stress is reduced from about 18 per cent to 4 per cent, yet this does not affect the strength of the ship. Will you please explain more explicitly how you arrived at this conclusion? A. I would like to correct the figures and statement slightly. Forty-five thousand pounds per square inch was the lowest that we found in any of the

tests that we made upon corroded material, including the corroded material from the scrap heap that we tested last spring. That does not mean to say that intercrystalline corrosion, continued further, would not reduce the strength even further than that, and in our present investigation, the lowest elongation we found was 5 per cent. The 4 per cent elongation that we have found was in corroded material from the scrap heap last spring. Five per cent is the lowest that we have found in this particular investigation.

The explanation as to why this does not weaken the structure of the ship, so far as static or aerodynamic loads is concerned, is contained in the statement that the design of the ship is so made that it does not utilize the full strength of the material, and anticipating a question of that kind I have brought with me a couple of specimen samples of duralumin to give a very simple illustration and by not what actually is in the ship but a similar case. These two pieces of duralumin; I know this one is not subject to intercrystalline corrosion, and this (indicating) is a piece of duralumin which I suspect very much has a slight attack of intercrystalline corrosion.

Q. If I may interrupt you a moment — are you willing to leave those with us? A. I am willing to leave them, yes.

Q. How do you identify these? A. This one happens to have on it a mark, 99.

Q. We will take this one and tag it. A. I hardly think there might be enough for that. I just want to show what they will do.

Q. You may proceed to illustrate. A. The point is that pieces from the same stock that these came from had been examined in some of our investigations and found to show intercrystalline corrosion, and in this one (indicating) they have been examined and found not to show intercrystalline corrosion, and if I take those two pieces, this and this (indicating) I should expect this to break, say, perhaps 2000 or 3000 pounds per square inch lower than this one. If I stand up and put a load on it like this (indicating) I would not ask it to stand a load of that kind. There is no difference in their strength, for the simple reason that that has failed. As soon as we flex it, it won't hold any more. If that material was corroded so badly that it would break about that time, then it would be weakened, but as you see that material is

not corroded badly enough so that it breaks at such a point. (The witness broke the piece of duralumin under his foot.) That has intercrystalline corrosion. It will show a lower elongation, but as long as I do not bend it enough to do something like that to it (indicating) or I do not pull it with force great enough to break it, if the structure of the ship is such that those will not bend in the ship, then the ship is just as safe with material of that kind in it, but if the corrosion goes so far that a bend of that kind (indicating) would break it, then the ship is not safe, and the answer is, of course, that the corrosion in the *Shenandoah* had not gone so far that any break short of something like that (indicating) — if I could show you that, I could show you some of the breaks that occurred, and no such bend as that could get into that ship without a girder having collapsed from other forces. I might also, just to illustrate the fact that that is true, show some photographs of girders in tests showing the type of failure. In one of those photographs, if the Court wishes to have them they are in the record, they may have shown tests of the girders of the *Shenandoah*, and if you look at them you can see how those girders bend like that (indicating). You take the load off from such a girder, and the channels straighten right out. If you break one of these girders, as this one was broken, by loading too far, it collapses. You take the load off this particular portion that is bent, and the rest of the girder is just as good as it ever was before, because we have taken and cut hundreds of them and re-tested them. The failure of these is by flexure and not by breaking the material.

Q. You have explained that the strength of the ship is not reduced by intercrystalline corrosion under static and aerodynamic loading. Is the same true when the ship is exposed to impulsive forces which require that the structure shall absorb a certain amount of energy? Is not the resilience lowered by the reduction in alternate strength and ductility, and isn't it possible that for that reason the power of the ship to absorb shocks and impulses will be reduced? A. That all depends upon what the shock and what the impulse is. If I should take a corroded piece of material and strike it by a very sharp hammer or an axe, its resistance to such a blow would be reduced. But if I could strike it with a

hammer enclosed in a piece of cloth wound around so that the shock was thereby softened or cushioned, I might strike it four or five times as hard a blow with that cushioned hammer and it would not break. Aerodynamic forces, no matter how impulsive they are, are cushioned much more by the very nature of air forces than I could obtain by any cushioning of a hammer, and the impulsiveness of air force, aerodynamic force, must be thought of as the impulsive force applied through a very resilient cushion, and that will give time for the energy absorption to take place in the same way, even under impulsive aerodynamic force, as they would take place as if those were statically applied air forces, and the absorption of energy by these girders is not by tension, but by the wavy flexure of the side which gives way. If I press on this side (indicating) the stretching side does not stretch hardly at all, and it tends to receive the energy like a spring. The other side bends in, if I push on it, like a spring and the other one stretches out taut and does not receive stresses sufficiently to break it. If a portion of the ship had struck suddenly upon a hard surface, as upon the ground, I should expect that a blow of that kind would have found the ship considerably weaker because of the intercrystalline corrosion, but any force which the air would possibly put on it — because the air is a cushion, that is all — though it may exert very enormously great force, would not weaken it.

Q. Does the reduction in fatigue strength of duralumin by intercrystalline corrosion have any influence on the lifting energy of the rigid airship built of this material? A. That is a hard question to answer. I do not believe that in the ship as constructed any vibratory stresses, approximately 10,000 pounds per square inch, could by any possibility be brought in. I do not believe that by the rotation of the engines — and those are the only ones which would bring any vibratory stresses of a high enough frequency to amount up to the millions upon millions which are necessary to break it — I do not believe, I do not know, but I do not believe, that any such stresses ever existed in the *Shenandoah* and consequently I should know that the corrosion would lessen the life of the ship by corroding ultimately clear through the material before it would have failed by fatigue. That is my belief, I do not know.

Q. In your opinion, Doctor, is there any connection between the treatment of sheet duralumin in its manufacture and the subsequent tendency to corrosion? A. There seems to be. I do not know as much about it as I would like to know.

Q. If so, what in your opinion is the proper treatment of the material in order to avoid any ill effects of the treatment, and in what respect did the manufacture of the material of the *Shenandoah* differ from what you consider the ideal treatment? A. Well, I wish I could answer that question. The investigation which the Bureau of Standards is now carrying out at the request of the Bureau of Aeronautics and the National Advisory Committee and the Army Air Service, we hope, will give an answer to that question. There seems to be some indication that if the material is cold-worked, after the last heat treatment, that it renders it more liable to corrosion. Now, if that is so, then the stretching of the channels of the *Shenandoah,* which we used to straighten them, increased their likelihood to corrosion. On the other hand, the lattices of the *Shenandoah* were also corroding, though not so rapidly, and they were heat treated after the last cold-working.

Q. When you say stretched, you mean stretched after heat treatment. A. The channels were heat treated, and then before they aged they were rolled into shape, and then put into a machine and stretched from a quarter to a half of one per cent, just enough to make them straight. That is, instead of having slight twists they could be pulled and then they would be stretched — that was at the time supposed to be very good practice, because at the time no records of the existence of intercrystalline corrosion were, so far as I know, in the literature of this country or any of the countries abroad.

Q. What is the difference, if any, between the dimensions and form of the test pieces used by the Bureau of Standards and those used by the Aluminum Company of America? A. I do not know what the exact shape of the aluminum specimens which were used by the Aluminum Company of America are. The shape of the test specimens makes some difference in the results of tests. They make some difference in the elongation, and they make some difference in the ultimate tensile strength, less in the tensile

strength than in the elongation, and considerable discussion has been had as to what is the proper shape of a tensile test specimen. As to the comparison between the Bureau of Standards and the Aluminum Company of America, I can only say that our test specimen was three-quarters of an inch wide, two inches gauge length, and a half inch thick — the reduced section was three inches long and the curvature at the junction was one and one-half inches all over. This gave the results on new material which checked with the report on elongation and tensile strength given by the Aluminum Company of America.

I have discussed in the report the difference between those tensile specimens and the type of tensile specimen we were able to secure from direct materials. The maximum reduced section we were able to obtain was one-half inch, and those were not straight, they were not true. They were as straight and true as they could be cut from the material, and consequently we expect that the tensile strengths obtained from them would be slightly lower. For instance, if the specifications said 55,000 pounds per square inch, and I found 53,000 pounds per square inch, I would say "Perfectly good." That is equal to the specifications because my test specimen must be imperfect. If the specifications said 18 per cent elongation and I found 16, I would say "Perfectly good." I would say that this was not a perfectly straight and true specimen. I wish I could get one, but I cannot; and therefore I am satisfied with this. I would say that that material is good material. But when I find 45,000 tensile strength and 5 per cent elongation, and at the same time my micrographic analysis says that there is a patch of intercrystalline corrosion where the specimen broke, those differences are not due to differences in the test specimens.

Q. In your judgment, could you get a sound small test piece from a channel or from a lattice? If not, how do you overcome this difficulty? A. That is fairly well answered, I think, by the other. We took our specimens from the channels or the lattice where we could get a flat and reasonably straight portion in a channel; where the channel was bent over the sides we would cut the reduced section, and then bend back the outside where the grips would come to hold it, and then we would make the edges just as straight as we could and we would measure the area of

the edges just as accurately as we could, and then pull. That is not as good as the new specimen, certainly not. Similarly, in the modulus test, where I tried to get a modulus material, I said that the modulus is the same in the corroded material as in the other. My modulus specimen was not straight, it was not true. It was as straight and true as I could get, and I found discrepancies amounting to something over 1 per cent in stretching to determine upon some specimen; whereas I found no difference as great as 1 per cent on a perfectly new specimen, but that is something which you have to expect in material of this kind.

Examined by an interested party:

Q. (Lieutenant Mayer.) In your examination of this material, Dr. Tuckerman, have you compared that with similar varnished specimens that have been used in exposure tests at the Bureau of Standards, with reference to determining whether corrosion is apparent in those specimens? A. We have no varnished exposure tests at the Bureau of Standards. We have unvarnished exposure tests at the Bureau of Standards.

Q. Do they show signs of intercrystalline corrosion? A. They do, more marked than in the case of the *Shenandoah*.

Q. Then the varnish, has it or not a slowing up effect? A. I feel very sure that it has, because the *Shenandoah* specimens are nowhere near as badly corroded as the exposure tests we have on the roof of the Bureau of Standards.

Q. Are those exposure tests to the atmosphere on the roof? A. To the atmosphere on the roof, yes. You see, they have been there practically as long as the *Shenandoah* has been in existence.

Q. I was aware of that fact. I merely wanted to bring the information to the attention of the Court. A. Yes, sir.

Q. From your experience with duralumin, would you state just briefly what effect the working of the material has on the tensile strength, the elastic limit of the elongation, just in a few words? A. The working of the material after this heat treatment increases the tensile strength, increases the yield point, and decreases the elongation.

Q. After we have material in that condition, what effect has ageing upon those qualities? A. The ageing increases the tensile strength, increases the yield point, and reduces the elongation.

Q. With respect to the test specimen used, it is obvious that a small type of specimen had to be used in order to obtain specimens from certain of the material which you investigated. Were similar tests conducted with the same sized specimen on new material at the same time that the *Shenandoah* material was tested? A. No.

Q. Then we have no exact comparison between new material with the same sized specimen? A. I do not know how I could make a test specimen which would be comparable.

Q. I mean, comparable to one used in this test? A. I do not know how I could form one and bend it so that it would be comparable, the particular point being that in such a test we know from other experience that changes in shape do not produce more than one or two thousand pounds per square inch difference in tensile strength. Consequently, if we are interested only in a difference of one thousand pounds per square inch, the extra time and labor in preparing those specimens would be wasted.

Q. But the elongation will vary a little more than that percentage, won't it? A. I should say not. I think I could get 16 per cent out of these specimens, out of new material which had 18 per cent, I mean.

Q. And you also mentioned the fact in your testimony that one skilled in the knowledge could by visual inspection determine whether and to what extent corrosion had taken place. Am I correct in that? A. I say that I think I could. We have not checked that up. That is to say, that is a conviction which has grown, and I hope that I will be able to check that up with more careful tests.

Q. That is not a matter of common knowledge, then? A. No.

Q. It has not been issued in any form? A. It has not been issued in any form, and a month ago, when I started this investigation, I would have said that I could not tell it.

Q. Have you found whether the rate of corrosion is accelerated or delayed? If you plot a curve showing the rate, has it a stopping point somewhere? A. We have no adequate knowledge of that. So far as any material which I have tested at this time from the *Shenandoah* is concerned, it has about the same amount as the material which I tested last spring, which came from the nose of the *Shenandoah* torn off in the wreck. That is, not knowing of

the six months' difference, or whatever the difference was between those two times, I would not be able to say one was more or less than the other. It is not going fast.

Q. Do your tests show that you can determine whether the life of an airship would be five or ten years? A. No.

Q. It is not that definite? A. No. The ship was safe the day she went down, as far as corrosion is concerned. Whether she would have been five years from now is not known.

Q. You quoted certain figures in your testimony — I jotted them down here — of material from the minimum tensile being 45,200 and minimum elongation 5 per cent. Then, corresponding to that, I think you quoted new material as having an alternate of 55,000 per square inch minimum, with a minimum elongation of 18 per cent? A. Yes.

Q. The first tests presumably were taken from lattice and channel materials; is that correct? A. You mean the first tests? Yes.

Q. The second were taken from straight sheet? A. Straight sheet material, yes.

Q. Then the differentiation in these properties, namely, alternate tensile strength and elongation, is not shown as between new sheet material and new rolled material? A. This is rolled sheet.

Q. Yes, sir. But I am referring to material after it has been made into the channel and the lattice? A. No.

Q. Will you please state what you would expect to be those corresponding figures with the samples that have been taken from channel or lattice material in a new condition, if that is possible? A. The lattice material I should expect to show standard specification values; that is, heat treated after it is stamped. That is, the new lattice material. Of course, to get from a lattice after it is stamped a standard specification is impossible. The only place you can get a specimen is off the sides of the lattice, and that is somewhere, I think, around three to four-tenths of an inch. So you would expect your elongations a little bit low. Your tensile strength you would expect to be between 55 and 65 thousand per square inch. If you had rolled your sheet into a channel, I should expect it to be toward the upper limits; that is, nearer the 65,000

than the 55,000. You see, the new material ranged from 18 to 24 per cent and rolled that way I should expect some of it probably to run a little bit low, probably 15, 16 perhaps, but it would run from 10 perhaps up to 18 or 20 per cent. It would run to the lower end of the specified value rather than to the upper end.

Q. In other words, the material is similar to steel in the way the properties vary after working. That is, the ultimate strength and the elastic limit are increased and the elongation is slightly decreased? A. Yes.

Q. If you were to be informed that certain of the lattices on the *Shenandoah* were worked after heat treatment, that would change your assumption with reference to the properties, would it not? A. It would change my assumption, yes. I would like to know which ones were worked after heat treatment, and I would like very much to know that, so I could check up as to the corrosion phenomenon which we found on the lattice.

Q. Do you know of any tests being conducted to show that corrosion is accelerated after heat treatment in duralumin — systematic tests? A. The Bureau of Standards is now conducting a systematic series of tests to determine whether the heat treatment, chemical composition, working or what not, or anything in the history of duralumin that has any influence on intercrystalline corrosion — that is, the investigation which is mentioned in the report which was instituted by the Bureau of Aeronautics and is now being carried on in co-operation with the National Advisory Committee and the Army Air Service and the Bureau of Aeronautics.

Q. In your statement of the general conclusions arrived at, I think you said modulus of elasticity of the material had not been impaired; in other words, that the material which you tested was comparable with new material? A. Yes.

Q. And therefore the strength of the ship had not been impaired, due to the fact that on the type of construction used in the *Shenandoah*, the modulus of elasticity was the only measure of the material? A. The modulus of the elasticity and the geometrical shape of the specimen.

Q. That cannot change the shape? A. No; the corrosion does not change the shape.

Q. You further stated that vibratory stresses from 24,000 pounds per square inch — that is vibratory stresses, if imposed upon the structure, the material would not be affected by such stresses? Is that the substance of your testimony? A. I should say plus or minus 20,000. It is a little bit hard to say. It is certainly plus or minus 10,000. I should say there was nothing in the life of the ship — how many hours has the ship been running?

Q. About 750. A. That would be less than a hundred million, or around a hundred million alterations of stresses, and there is nothing in our tests which would indicate that any stress less than plus or minus 10,000 would cause any material that we have ever tested to fail under those circumstances.

Q. I understood you to say 24,000, the purpose being that was, as you know, the strength of the girders in the ship are less than that figure, and what I was trying to bring out was whether in your opinion if a girder was designed to have a failing stress under a critical vibratory stress, the vibration would really never enter into it at all. A. I do not quite get that. (The question was read by the reporter.) A. (Continuing.) Perhaps you have misunderstood. It is a range of stress of 24,000, from minus twelve to plus twelve thousand. That might occur.

Q. (By Commander Rosendahl.) Were you a member of the special technical committee that investigated the design of the *Shenandoah?* A. I was.

Q. What was the general condition of the material from the *Shenandoah* structure which you examined as regards its cleanliness and the evident care that had been taken of it? A. That is rather a hard question. The only two types of dirt that I found, or rather three types of dirt I found, were that very obviously birds had been somewhere around the upper girders at the top of the ship at some time or other. Then there was the dirt that presumably came upon it from being in contact with the ground. And then there was a slight amount of dirt that had accumulated on the material. Otherwise, the material seemed very clean.

Q. (By Lieutenant Whittle.) Dr. Tuckerman, how do you account for intercrystalline corrosion occurring in material coated with spar varnish? A. I do not know it does.

Q. (By the Court.) I note on page 3 of the report, Doctor, that you state "One showed a tensile strength of 45,000 pounds per square inch, while the others ranged from 53,500 to 63,200." On page 1 you state that 29 tensile tests were made? A. Yes.

Q. That means that 28 of them lay between the 53,000 and the 63,000? A. Yes.

Q. And only one fell outside of that range? A. Yes.

Q. Similarly, you say the percentage of elongation varied from 5 per cent to 17½ per cent. Can you give us any idea, in that 29 specimens on which that percentage elongation was based, as to where the greater portion of the specimens are found? A. If you will let me take that report I will refer you to that.

Q. I would like to bring that a little more clearly onto the record. A. One at 5, one at 6, one at 7, one at 7½, one at 8, two at 9, two at 9½ — approximately a third of them less than 10 per cent, ranging all the way from 5 up to 10 per cent. The rest are between 10 — about equally distributed from 10 up to 17½ per cent.

Q. A more or less uniform distribution? A. A more or less uniform distribution, the majority being above 10 per cent. I might add also that this was the result of a search for the very worst material that I could find. These 29 tensile specimens we had; we had tried to take 200 odd tensile specimens at the time. In the first place, we could not have gotten them, and in the second place, each one of them has to be most carefully cut out by hand, otherwise you will damage it and the test is not worth anything. So, to take anything near the number of tensile specimens we wanted would be out of the question. We took specimens adjacent and bent specimens so as to compare these results, and this reduced our hunt for the very worst material we could find for these specimens I picked. You see, these are patches; they are randomly distributed, and sometimes you will get a patch that may bring the elongation down to 5 per cent; sometimes you may bring the elongation down to 7 per cent, and sometimes you will get a little patch — and those patches only go about two- and at the most eight-thousandths of an inch into the material which we have found — good material; though, of course, it is unfortunate to have material that you cannot rely upon for an indefinite life.

Q. That statement brings out something that I want to ask. Dr. Tuckerman, you have stated that nothing you found in the condition of this material would have, in your opinion, affected the strength of the ship at this time. You base that not only on the result of your material tests, but on your knowledge of the design of the ship as gained from your previous service upon the committee to which you have testified you were a member? A. Yes, and also from the tests made at the Bureau of Standards on actual girders taken from the ship, during the construction of the ship.

Q. Your further testimony in regard to the corrosion observed is to the effect that the state of energy in regard to this material at the present time is not such that the probable life of such structures built on this principle from this material can as yet be predicted. A. I would not want to build a structure of this material at the present time, that I would expect to last over ten years. I would be perfectly willing to build a structure of this material which I expected to serve its useful life in ten years, but I would not want — for instance, I would not want to build a bridge that I expected to last 60 years out of it.

Q. As a result, Doctor, of your investigations on the material submitted to you from the *Shenandoah*, was the structure of the ship in your opinion materially weaker than her designed original strength? A. No.

BUREAU OF STANDARDS REPORT.

SUPPLEMENT TO REPORT ON TESTS OF MATERIAL FROM THE U.S.S. "SHENANDOAH" SUBMITTED BY BUREAU OF AERONAUTICS, NAVY DEPARTMENT.

A conference was held at the Bureau of Standards on February 5, 1926, to discuss the "Bureau of Standards Report on Tests of Material from the U.S.S. *Shenandoah*," and consider the desirability of further tests. As a result of this conference, a series of supplementary tests were planned which are now in progress, but not yet completed.

The discussion at the conference brought out no facts which could alter the conclusions arrived at in the original report.

However, evidence was submitted to the effect that the original percentage elongation of the duralumin in the channels of the *Shenandoah* might have been appreciably lower than the 18 per cent specified for sheet material in the acceptance tests.

The acceptance tests were specified to be made on the material in sheet form in the heat treated and aged condition before forming into channels, and the Bureau of Standards tests of new material were made on material in sheet form.

In forming into channels the sheet was heat treated, passed through forming rolls, stretched approximately ¼ to ½ per cent to straighten them and then aged. In this process considerable cold work was done upon the material which should accordingly be expected to give somewhat different results than the sheet from which it was formed. No acceptance tests were made on the channels themselves.

Consequently the statement (Bureau of Standards Report above), "Bureau of Standards test on *comparable* (no underscore in original) new material gave results varying from 55,000 to 65,000 pounds per square inch. The percentage elongation in two inches varied from 5 per cent to 17½ per cent, where *comparable* (no underscore in original) new material varied from 18 per cent to 24 per cent," is not accurate, since a comparison is drawn between materials which were not wholly comparable.

This comparison, although in no way affecting the conclusions with respect to the *Shenandoah*, might lead people to form a judgment of the length of the useful life of duralumin manufactured under these Navy Specifications, a judgment not justified by the evidence and which might be much modified by the results of the supplementary tests now in progress.

No conclusions as to the probable useful life of duralumin based upon a comparison of the results of the tests on the material from the *Shenandoah* wreck with new material are at present warranted.

The fatigue tests planned (Bureau of Standards Report above), have since been completed. They showed no specimen with as low fatigue resistance as the least resistant of the corroded material

previously tested and a number more resistant than any of the material from the scrap heap. The conclusion (Bureau of Standards Report above), "This investigation, therefore, furnishes no evidence that fatigue failure contributed to the wreck of the ship," is therefore confirmed.

GEORGE K. BURGESS, *Director.*

Washington, D. C.
June 15, 1926.

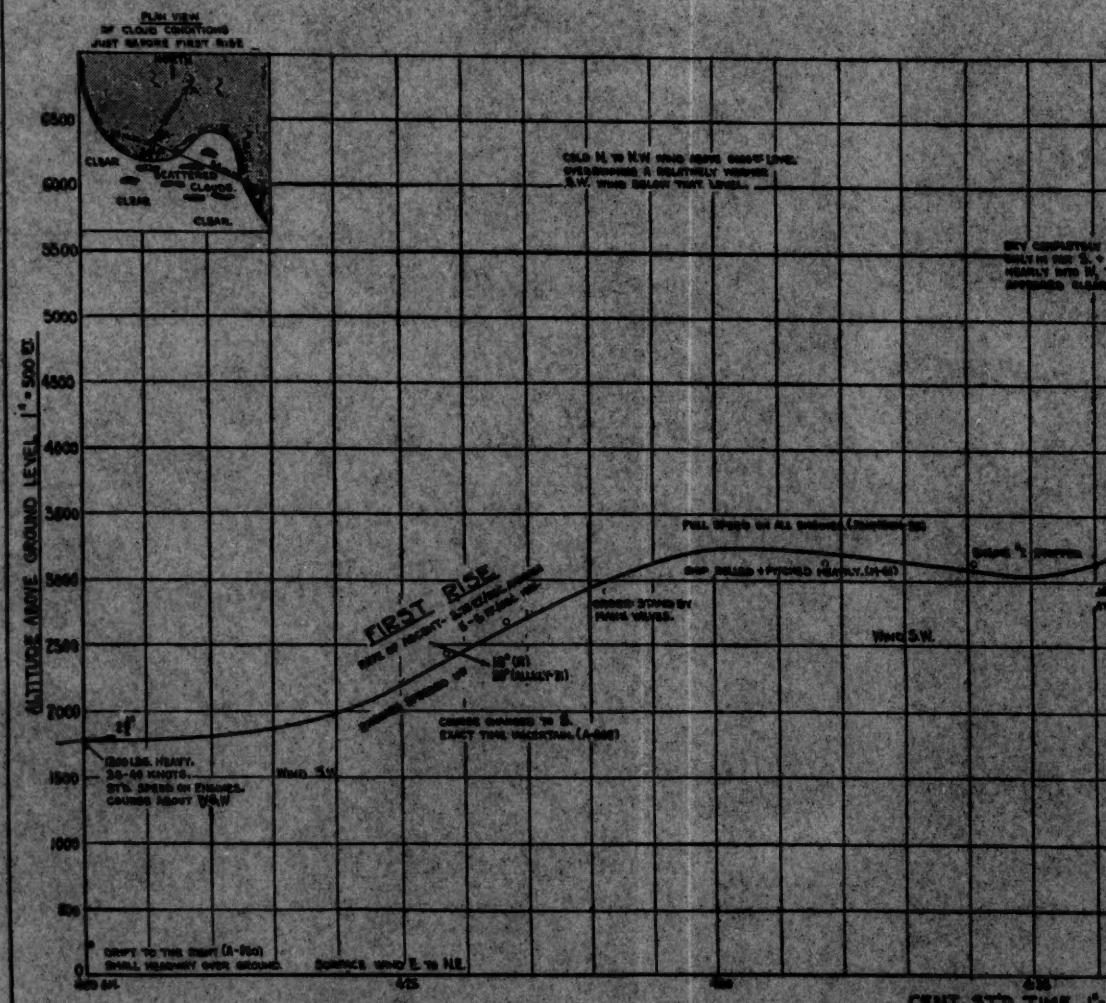
TECHNICAL ARGUMENT

BY PROFESSOR WILLIAM HOVGAARD, TECHNICAL ADVISER TO THE JUDGE ADVOCATE.

INTRODUCTION.

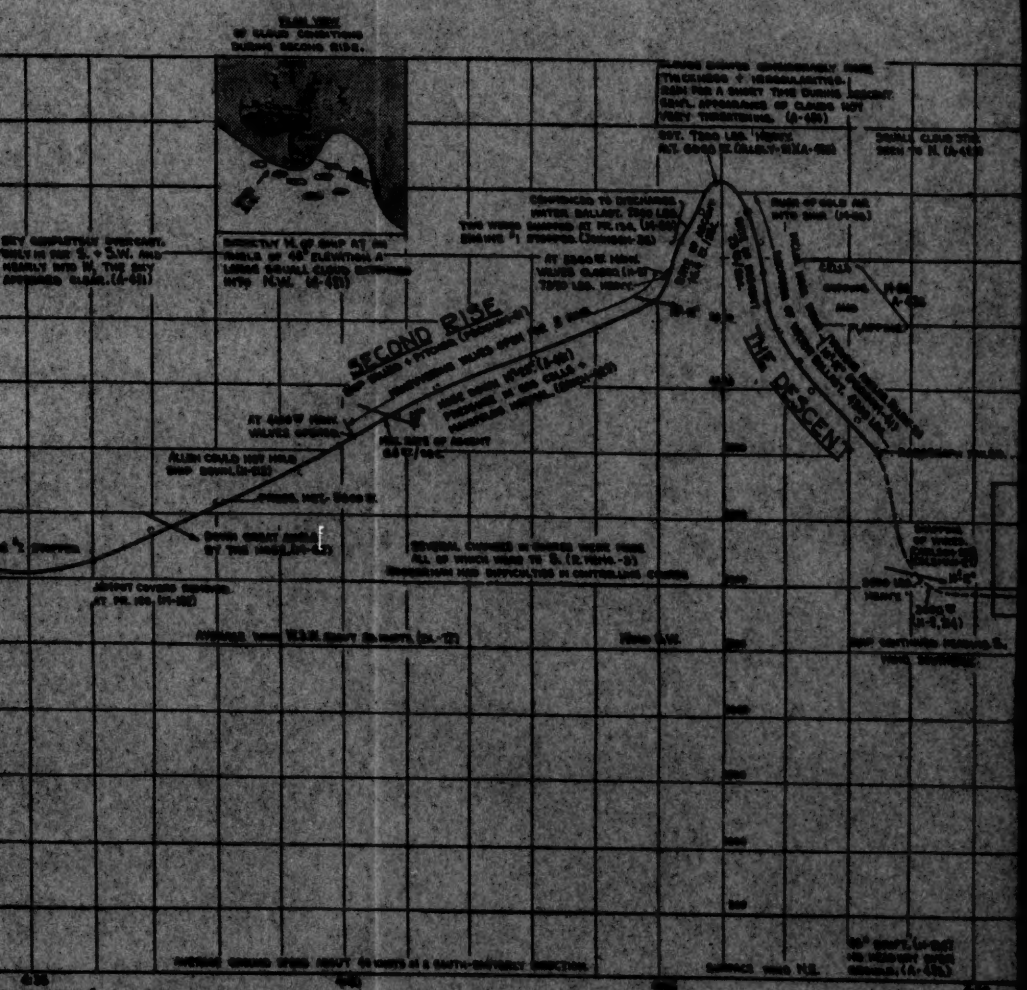
A discussion of matters pertaining to organization and operation of the *Shenandoah* and responsibility for the loss is regarded as outside the scope of this report, which deals only with the technical aspect of the accident.

Various data pertinent to the inquiry, in particular, the sequence of events and the altitude of the ship from 4:20 a.m. (Central Standard Time) on September 3, 1925, until the loss of the ship at about 4:52 a.m. on the same date, are represented on the accompanying Chart, prepared under my direction in the Bureau of Aeronautics. On this chart the altitude is plotted on time and the scales are so chosen as to give space for a statement of the principal events. A special chart to enlarged scale covering the events during the last rise of the ship is added for the sake of clearness. The curve of altitude is obtained from the barograph record up to about 4:48 a.m., when this instrument failed. The readings were taken from the barograph record under a magnifying glass with the utmost care and were checked by readings from an enlarged photographic copy of the record. They were interpreted by means of a comparison with the altimeter readings corrected for barometric height as given in an exhibit submitted to the Court by Lieutenant C. H. Havill, U.S.N. The corrected altitudes so obtained are only from one to two hundred feet smaller than the altimeter readings.


PLAN VIEW
OF CLOUD CONDITIONS
JUST BEFORE FIRST RISE
NORTH
CLEAR
SCATTERED
CLOUDS
CLEAR
CLEAR
6500
6000
5500
5000
4500
4000
3500
3000
2500
2000
1500
1000
500
0
ALTITUDE ABOVE GROUND LEVEL 1" = 500 FT.
COLD N. TO N.W. WIND ABOVE GROUND LEVEL
OVERRUNNING A RELATIVELY WARMER
S.W. WIND BELOW THAT LEVEL.
FIRST RISE
RATE OF ASCENT- 5 FEET PER SECOND
0-5 FEET PER MIN.
CHANGE GAINED TO E.
START TAXYING
WIND S.W.
WIND S.W.
DRIFT TO THE EIGHT (A-100)
SMALL HEADWAY OVER GROUND
SURFACE WIND E. TO N.E.
4/15
CENT. STD TIME

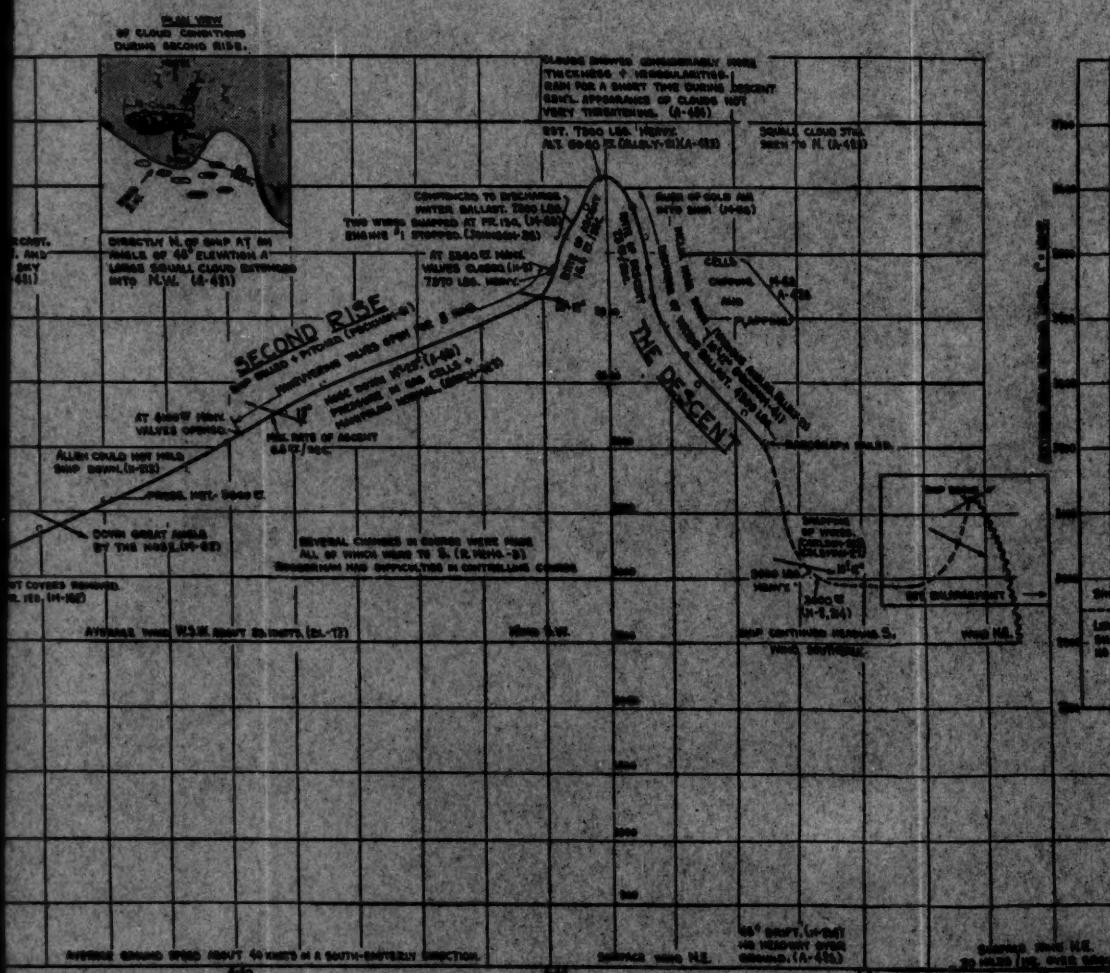
FLIGHT OF SHENANDOAH ON SEPT. 3, 1925, FROM 4:20 A.M. UNTIL TIME OF HER LOSS.

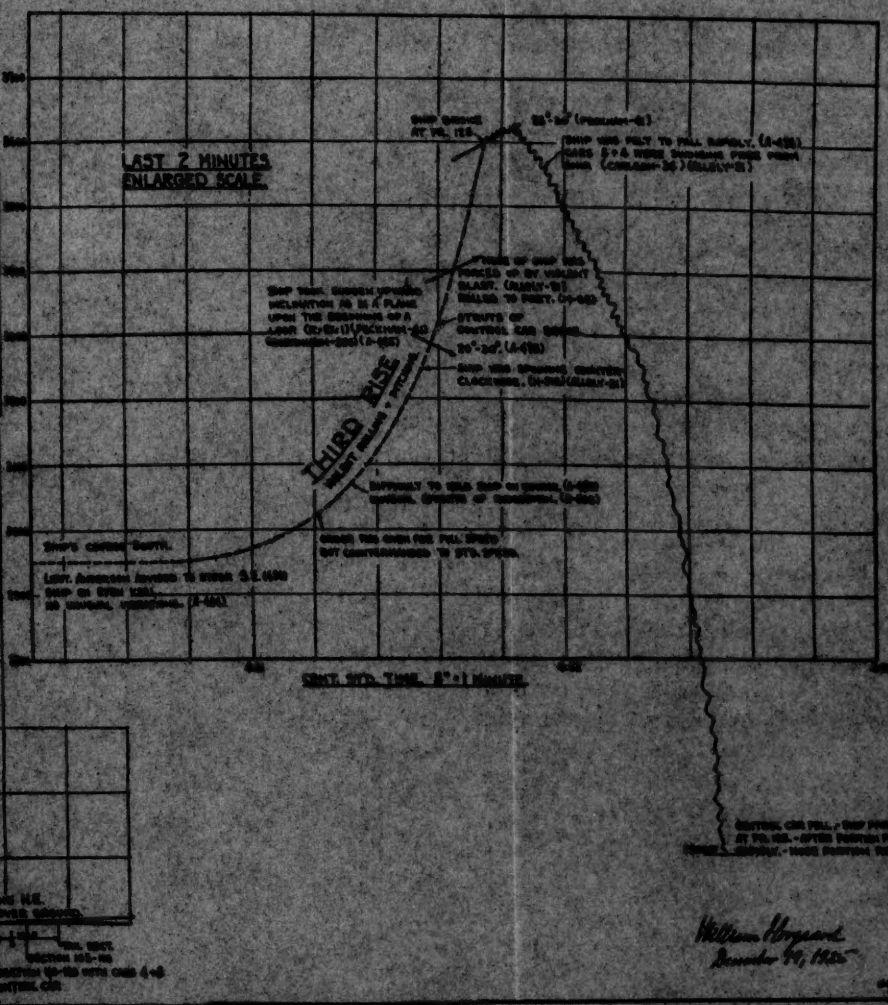
CURVE OF ALTITUDE WITH EVENTS PLOTTED ON TIME.



FLIGHT OF SHENANDOAH ON SEPT. 3, 1925, FROM 4:20 A.M. UNTIL TIME OF HER LOSS.

CURVE OF ALTITUDE WITH EVENTS PLOTTED ON TIME.





William Hoggard
December 29, 1925

GENERAL ACCOUNT OF EVENTS.

At about 4:20 a.m. the *Shenandoah* was going with engines Nos. 1, 2, 3 at standard power, and Nos. 4 and 5 at two-thirds power. The course was somewhat north of southwest, and the air speed of the ship was about 40 knots. The wind was southwesterly and the ship made but small headway over the ground. There was a drift to the right. The ship was 1200 pounds heavy and nosed up some 3 degrees. The pressure height at that time was about 3600 feet (3800 feet on the altimeter).

There were eight automatic valves, namely, on cells Nos. 4, 5; 8, 9; 12, 13; and 16, 17. Hand on maneuvering valves were fitted in cells Nos. 4, 6, 7; 10, 11; 14, 15; 18.

The automatic valves in cells Nos. 8, 9; 17, 7 were open, but the remaining four automatic valves were closed by jampot covers provided with rip panels. The interconnecting gas tubes were all free, all stops having been removed from the inflation line before midnight.

THE FIRST RISE.

The altitude was about 1800 feet when at 4:23 a.m. the ship met an ascending air current, which caused her to rise suddenly.

It was attempted to check the rise by means of the elevator and the ship was made to nose down about 18 degrees. The rate of rise averaged about 3.75 feet per second, and reached a maximum of about 5 feet per second.

At 4:30, when an altitude of about 3100 feet was reached, the ascent was checked and the ship continued on nearly the same level for about six minutes. The pressure at the bottom of the cells was still very small. It may have been as much as 12½ millimeters (.5″), but this would have no material influence on the amount of gas subsequently valved. Order was given for full power on all the engines, but shortly after engine No. 2 stopped running. The failure of engine No. 2, as also that of engine No. 1, which occurred soon after, is ascribed by some to the great inclinations of the ship, which inclinations caused improper bearing lubrication and hence overheating of the bearings and of the cooling water. The jampot covers were removed or ripped from

the automatic valves in cells Nos. 4, 5, 12, 13, so that henceforth all the valves functioned freely. None of the manifolds nor any of the sleeves, which connect the manifolds with the gas cells, were tied off at that time.

It appears that some time during this rise the course was changed to south, and as the wind was from west-southwest and blowing with a velocity of about 30 knots, the ship now made a good speed, perhaps about 40 knots, over the ground in a southeasterly direction. This condition probably lasted throughout the second rise.

THE SECOND RISE.

At 4:36 the ship began a second rise of greater magnitude and steepness than the first. The pressure height was soon passed and the rise continued up to an altitude of about 5000 feet at an average rate of 3.75 feet per second, reaching at times about 6.5 feet per second; but at 5150 feet a much steeper rise commenced, which carried the ship up to 6060 feet (6200 feet on the altimeter), and averaged 16.4 feet per second (5.0 millimeters per second) or nearly 1000 feet per minute. The peak was reached at 4:46 a.m.

During this rise the ship was inclined down by the bow at an angle which at first was about 18 degrees, but towards the end of the rise seems to have been some 10 degrees or 12 degrees. In order to check the rise and prevent excessive gas pressures being formed in the cells, the maneuvering valves were kept open for five minutes, beginning when the ship was at an altitude of 4100 feet and closing at an altitude of 5360 feet (altimeter in Control Room showed 5500 feet, as observed by Colonel C. G. Hall).

There is no evidence to show that the maneuvering valves were not fully open as intended or that excessive gas pressures existed at any time during this rise. At the time when the maneuvering valves were closed, the cells fitted with automatic valves had a calculated pressure at the bottom, calculated by Mr. C. P. Burgess to be of .4 to .5 inches (10 to 12½ millimeters), while the cells with maneuvering valves were estimated to be about 97 per cent full. At this time there were about 9400 pounds of water ballast on board.

The rise from the time that the maneuvering valves were closed until the maximum altitude was reached occupied 42 seconds, during which the ship rose 700 feet, or at an average rate of 16.7 feet per second (5.1 millimeters per second). During this period the automatic valves functioned and it was calculated that when the peak was reached the pressure at the automatic valves was 4.9 pounds per square foot and at the top 9.0 pounds per square foot, reckoning a lift of the gas of .049 pounds per cubic foot. After the maneuvering valves had been closed, but before the peak of altitude was reached, order was given to drop ballast water, and it appears that toggles for the release of water from the bags were immediately pulled, and that before the peak was reached all the water ballast at amidships, about 2500 pounds, had run out. Thus, when the peak was reached, there was 9400 — 2500 = 6900 pounds of water ballast on board.

The loss in buoyancy due to the rise in altitude from pressure height to 5360 feet was about 6170 pounds, and at the peak it was about 6600 pounds.

Hence, the total overweight when the maneuvering valves were closed was: 1200 plus 6170 equals 7370 pounds, and when the peak was reached 1200 plus 8600 minus 2500 equals 7300 pounds.

Shortly before the peak was reached, two wires were heard to snap somewhere in the upper part of the ship. The sound was heard distinctly by Lieutenant R. G. Mayer, who estimated it to come from about frame 130. At the same time a sound like the breaking of one or more girders was heard by Masters, Aviation Pilot, who was at frame 75-80, and described it as being only a single report located above his head, that is, 160 feet aft of frame 130. Machinist's Mate, first class, Solar at frame 75 heard a wire snap when at the peak. Lieutenant Mayer also heard a distinct crashing sound when the ship was at the peak. He was at that time at frame 165 and heard the sound aft and to the port of him. He thought it was due to dropping of car No. 4, but this did not happen until later when the ship broke.

It is impossible to ascertain how many different breakdowns actually occurred, but from Lieutenant Mayer's testimony it is certain that before the peak was reached something gave way; at

any rate, two wires snapped, and at least one more breakdown occurred when the ship was at the highest altitude.

Engine No. 1 failed before the peak of altitude was reached, but before the final steep rise began it may be reckoned that engines Nos. 1, 3, 4 and 5 were running, and during this time the ship had an inclination down by the head of about 18 degrees. It is estimated that under these circumstances the air speed was 48 knots, equals 81 feet per second. As the ship was about 7000 pounds heavy and was yet driven upwards, the air current must have impinged on the underside of the ship in spite of her great downward inclination. The vertical component of the velocity of the air current is therefore found as the sum of three parts; first, the upward velocity of the ship relative to the ground, 3.75 feet per second; second, the part due to the downward inclination of the ship: 81 times sin.—18 degrees equals 25.0 feet per second; and third, the component necessary to sustain the ship against a heaviness of 7000 pounds, found to be 6.5 feet per second. Hence the total vertical component velocity of the air current was: 3.7 plus 25.0 plus 6.5 equals 35.2 feet per second, equals 10.7 meters per second, equals 24 miles per hour.

During the entire second rise, a period of about 10 minutes, the ship was, according to Lieutenant Mayer, pitching and rolling.

There is no evidence to show that any of the cells had burst or were deflated at this stage of the flight, nor was there any sign of excessive pressure. The manifold was tied off between cells No. 8 and No. 9, probably at the beginning of the second rise, in order to prevent gas from flowing from the forward to the after cells, as the ship was up by the tail.

THE DESCENT.

Immediately after the peak had been reached, the ship began to fall very rapidly, the rate at first, according to the barograph, about 25 feet per second, or 7½ meters per second. Probably the rate of fall eased off somewhat in the lower part of the descent and was finally checked about three minutes after the fall commenced.

Soon after the fall began, the cells commenced to cup up and flap and air was drawn in at high velocity through the openings pro-

vided for this purpose in the outer cover at the bottom of the ship. Lieutenant Mayer, who was at frame 170, felt a terrific rush of cold air through the forward ventilating hatches, giving evidence of the rapidity of the fall.

There were two regular ventilating hatches forward which were calculated to be sufficient to prevent excessive differences in pressure inside and outside of the outer cover when the altitude was changed. But besides the regular openings two bomb hatches and a number of automatic ventilating hatches had been opened on this occasion on account of the hot weather. Thus the total area of ventilating openings, which was normally 108 square feet, was augmented to 136 square feet.

During the descent the ship was inclined down by the head at varying angles, being sometimes almost horizontal. When the fall came to a stop, the ship was nosing down some 10 degrees to 12 degrees.

The suddenness with which the descent, according to Colonel Hall, was stopped can only be explained by the advent of a strong ascending air current, for in spite of the dropping of ballast the ship was still heavy. It is estimated that altogether 6800 pounds of water ballast had been dropped, of which, as stated above, about 2500 pounds were dropped before the peak was reached, leaving 4300 pounds which were dropped during the descent. Thus at the bottom of the descent there was still left 9400 minus 6800 equals 2600 pounds, and the ship was 7300 minus 4300 equals 3000 pounds heavy. The altitude at the lowest point has been estimated by some at 2500 feet. Aviation Chief Rigger Allely, who read the altimeter in the crew space during the descent, gives the altitude at the lowest point as 3500 feet. On the chart it is given as a little less than 3000 feet. At the time when the lowest altitude was reached Colonel Hall reports a drift of 45 degrees, but he does not then refer to any spinning motion in the horizontal plane.

Aviation Chief Rigger Carlson at frame 60, while the ship was falling, heard two wires snap on the port side in the lower part of the ship. As the ship leveled off at the bottom of the descent, Chief Machinist Mate Coleman at frame 90 heard a wire snap. Lieutenant T. B. Hendley, at frame 70, reports that he was awak-

ened by a gas cell, presumably No. 8, pressing down on his face, but the time for these events is uncertain.

THIRD RISE AND BREAKING OF THE SHIP.

It appears that the ship remained on the low level of approximately 3000 feet for one or two minutes, when suddenly the bow received a violent upward impulse by an air current, and now events began to happen in rapid succession. The sequence of these events cannot be determined with absolute certainty, but based on the testimony of those witnesses who are believed to be most reliable and who were in the best position for observation, the following account is suggested:

The gust caused a steep ascent of the whole ship, accompanied by strong vibrations and rolling motion, the nose turned to port, and the ship was set spinning around counter-clockwise in the horizontal plane.

The ship rolled over to port and Lieutenant Mayer, who stood in the keel at frame 170, was thrown violently over toward the netting on the port side of the keel. One or more of the struts holding the control car broke, and the ship, after having first pitched violently down, took a sudden upward inclination of perhaps as much as 30 degrees, giving the same sensation as in a plane at the beginning of a loop. Immediately following, perhaps some 10 or 20 seconds after the breaking of the struts, there was heard a loud continued crashing of breaking girders in the upper part of the ship somewhere between frame 120-130. About the same time, although not before the ship broke, car No. 4 was seen to swing down, but remained hanging in its after suspensions. In the meantime, the ship had been carried up some 700-1000 feet, being probably at 3500 to 4000 feet altitude when the break occurred.

At the time when the ship broke, it appears that cell No. 10 between frame 90 and 100 was deflated, and there can be no doubt that also cell No. 13, between frame 120 and 130, where the ship broke, must have been deflated.

Aviation Chief Rigger Collier saw cell No. 9 at least 30 per cent deflated and deflating very rapidly, and the next cells as far forward as he could see were also deflated, although he could not

see to what extent, but cell No. 8 was up to full pressure. At that time the ship was nosing up and in violent motion. A few seconds later Collier heard a motor stop, then dead silence for a moment, then somebody yelled (Cullinan?) and immediately after came the crash when the ship broke.

It seems certain that when the break occurred the middle portion of the ship was very heavy and tended to break away. Car No. 4 was hanging below cell No. 12, and the structure in this region must have been subject to very heavy strains. Presumably all the longitudinals broke in the frame space 120-130, but the after portion was prevented from falling away from the forward portion by the control cables, especially the major controls. These cables ran aft from sheaves immediately above the car and the tremendous pull exerted by the after part of the ship caused the sheave foundations to carry away, taking parts of the structure with them. The cables now cut through several of the transverses in the lower panels of the keel and the control car became subject to a strong aftward pull. The suspension wires broke away one after another, first those leading forward and then those on the port side. The control car swung down, rolled over, being suspended in the control cables and in the suspension cables on the starboard side. It was perhaps at this time, when the car rolled over, that the occupants were thrown out. Immediately after swinging down, the chains attached to the forward end of the control cables broke by gradual opening and stretching of the end links which connected them to the cables, and the car fell to the ground. This probably occurred within less than one minute after the breaking of the ship.

Now followed a general break-up of the ship into at least two separate parts. The after portion, which up to this time had been held together with the forward part by the control cables, parted from it at about 125. Being deprived of a great part of its buoyancy and loaded by the weight of cars No. 4 and No. 5, the after part tore away and started to fall at about the same time as the control car. A second break occurred at frame 110, but it is not known whether this took place at the same time or later. Perhaps it did not happen until the after portion struck the ground. That the break at 125 was simultaneous with the fall of the control car

is evidenced by the fact that the middle portion of the ship, together with power cars No. 4 and No. 5, fell to the ground at a distance from the control car which was only 38 feet greater than the distance between these parts as located in the ship. The discrepancy of 38 feet is accounted for by the fact that the after and middle portion, possessing some buoyancy, fell more slowly; in fact, so slowly that several men in the middle section landed safely on the ground. It is of interest to note that the control cables held the ship together for almost one minute after the break occurred.

The cables and the control car in tearing away caused a large hole to be formed in the lower part of the keel above the car. The outer cover under the bottom from frame 110 to 130 was torn away at the time of the break.

The after portion fell rapidly, at first inclining up by the tail some 30 degrees, but gradually leveling off to about 5 degrees before it came to the ground. After the middle section had broken away at frame 110, the tail portion drifted away in a southwesterly direction and landed about one-third of a mile southwest of the middle section. About half way, while drifting close to the ground, with a speed of perhaps some 20 knots, the forward part of the tail portion, frame 105, was brushed off on striking some trees and fell to the ground.

The forward part of the ship, from frame 130 to the nose, after the fall of the control car, rose rapidly to a great height, but by valving gas it was brought safely to the ground some twelve miles southwest of the place where the break took place.

The location of the various parts of the ship after they reached the ground throws considerable light on the sequence of the events and the fact that they all fell in a line roughly from northeast to southwest shows that the wind at this time, at the level where the ship broke, as well as at the ground, must have been northeasterly. About one-half of a mile northeast of the control car was found the body of one of the men, Cullinan, indicating probably the place where the ship broke. The fact that Cullinan could fall from the ship seems to show that the outer cover was at that time torn away under the keel. The time of fall from an altitude of 3600 feet would be from 15 to 20 seconds.

Estimating the speed of the ship from the time that the break occurred, when Cullinan fell, until the control car fell, to be about 30 knots, the corresponding time interval would be about 50 seconds or somewhat less than one minute, as stated above.

The bodies of the eight occupants of the control car, except one, fell in a scattered group outside and some 10 to 25 feet northeast of the car, showing that they had probably been projected out of the car when it rolled over, a moment before it tore away, although it is possible that they were thrown out during the fall or when the car struck the ground.

When the ship broke up, as described above, the air speed was probably checked and the tail drifted in the same direction as the wind and with the same speed. It is reported by several survivors that when near the ground the speed of the tail portion was considerable. If the wind velocity is estimated at 20 miles per hour, we find the time interval occupied by the tail in covering the last one-third of a mile to be about one minute.

The radio car was carried along by the bow portion for some 15 to 20 seconds after the control car fell. The pad eyes to which the struts of the radio car were secured on the M-and M′ longitudinals were wrenched sideways before they broke, showing the violent action of the transverse pins due to the rolling motion of the ship.

Upon landing, there was about 1600 pounds of water ballast on board the bow section and 1000 pounds on board the tail section. Hence there was dropped a total amount of water of 9400 minus 2600 equals 6800 pounds, or about 7000 pounds.

THE CAUSE OF BREAKING OF THE SHIP.

It appears that the principal cause of the breaking was a great sagging moment at frame 120-130, due to an upward gust, directed obliquely from the starboard towards the port side, and, due to the speed of the ship through the air, at a considerable angle with the transverse plane. This is consistent with the fact that the nose was turned violently upwards and to port and that the ship was set turning counter-clockwise in the horizontal plane, while she was carried bodily upwards some 500 to 1000 feet. It

is consistent also with the report of Lieutenant Commander W. Nelson that the longitudinals in the upper port side caved in.

In order to form an idea of the stresses produced in the structure by a gust of unusual intensity, Mr. Burgess made a calculation based on the assumption that a gust of 80 feet per second relative velocity struck the ship at an angle of 45 degrees with the axis. He found that such a gust would create a bending moment at frame 120 of about 600,000 meter pounds, which would produce a stress in the top longitudinal of 13,200 pounds per square inch. Since the crippling stress in the girders is about 20,000 pounds per square inch, outside the double latticing, this cannot by itself account for their breaking, and we have to examine whether the stress due to bending was augmented by stresses due to other causes acting simultaneously or whether perhaps the ship was in a weakened condition from excessive stresses previously suffered. It is proposed to examine the latter possibility first.

From the various testimonies it seems established that some of the wires broke when the ship was at or near the peak of altitude and that other wires broke at or near the bottom of the descent. It has not been possible to account for these facts by calculation on any reasonable assumption, provided all the valves functioned as they were intended to do.

It has been calculated by Mr. Burgess that when the ship was at the maximum altitude of 6060 feet, and assuming that the valves functioned properly, the gas pressure at the top of the cells was 8.0 pounds per square foot. This pressure would produce a tension in the netting wires of only 400 pounds and a compressive stress in the apex channel of the C-longitudinals of little more than 10,000 pounds per square inch, but the breaking strength of the wires is 1040 pounds and the crippling stress of the longitudinals is 26,000 to 27,000 pounds per square inch.

In order to explain the breaking of the shear wires, we shall tentatively assume that aerodynamic and inertia forces caused additional strains, but as it is not known that excessive local gusts existed when the ship was at the peak, since no violent pitching occurred at that time, there remains to consider only the bending moments caused by heaviness and inertia of the ship.

During the last steep rise before the peak of altitude was reached, the ship was about 7300 pounds heavy. At the moment when this rapid rise began the ship had an upward acceleration which, judging from the barograph curve of altitude (see Chart), was about 1 foot per second per second. The barograph curve as given on the chart of the instrument is, however, to an exceedingly small scale, and it is possible that much greater accelerations may have existed for short periods. Violent upward gusts of short duration may have occurred, and are, in fact, indicated by certain irregularities in the barograph curve. An upward acceleration of 2 feet per second per second may have existed and would require the action of an upward force of about 2/32.2, or about one-sixteenth of the weight of the ship. Taking the weight of the ship equal to 136,000 pounds, we have an upward force of 8500 pounds, which, if added to the force required to balance the heaviness of 7300 pounds, gives a total of 15,800 pounds. This would produce a sagging moment of about 300,000 meter pounds. Deducting the existing static hogging moment of 195,000 meter pounds, we have a resultant sagging moment of 105,000 meter pounds, but even this could not account for an overstrain of the structure.

At the peak, where the velocity changed from being upward to downward, the downward acceleration was found to be less than 1 foot per second per second, and this is more than accounted for by the heaviness of the ship due to gravity, showing that at this point the upward current probably persisted but fell off considerably in intensity.

Similar calculations for the descent of the ship fail to reveal any plausible cause for the snapping of shear wires.

As regards the occurrence of *excessive gas pressures* in any of the cells, all witnesses who had an opportunity to inspect or notice the condition of the cells and manifolds are unanimous in stating that there was no evidence of excessive pressure in any of the cells at any time. Collier's testimony, referred to above, does not necessarily imply excessive pressure.

There remains the possibility that some of the *shear wires* may have been accidentally under a very high initial stress, since it is impossible to tune the wires to a perfectly uniform tension, and

the violent pitching and rolling, the quick accelerations and possibly twisting to which the ship was subject may have caused local breakdown of these wires.

If netting wires snapped, the bulge of the respective cell would be increased and the radial load on the adjacent longitudinals would be thus materially augmented. Under the rather severe assumption that the bulge of the cell increased by 6 inches, it would come to bulge practically against the outer cover and a stress of about 19,000 pounds per square inch would be then produced in the apex channel of the C-longitudinals. This is near the failing stress of the channel, but the assumption is rather extreme.

It remains to examine whether *the external pressures on the outer cover* under the rapid descent can have caused any damage to the structure. It was stated above that the total area of the ventilating openings was 136 square feet, and it is calculated that even with a coefficient of discharge as low as .5, allowing for frictional resistances, the excess pressure on the outer cover would only be about .5 pounds per square foot. Assuming a bulge of the cover of 1.8 inches between longitudinals, the radial load on the girders due to gas pressure is calculated to be 14.35 pounds per foot. This would produce a stress in the apex of the C-longitudinals of 2660 pounds per square inch, and in the base of the J-longitudinals of 3680 pounds per square inch. If the cover was very tight, these figures might be doubled. The stresses due to this cause are greatest in the lower longitudinals, because here the external pressures are not counteracted by the gas pressures, as they are in the top of the ship. They are not, however, of such magnitude that they can have caused any permanent strains in the girders.

We have thus to admit that we are unable to account satisfactorily for the snapping of either the shear or netting wires, and although it is probable that breakdown of this nature did occur, we are not justified in asserting that the structure was thereby materially weakened. We shall therefore examine if it is possible to account for the final breakdown of the ship without predicating prior weakening of the structure.

It was stated above that an upward gust of *80 feet per second* or *24 meters per second,* (55 miles per hour), would produce a compressive stress of 13,200 pounds per square inch in the top longitudinal, which stress is far below the crippling stress.

The first possibility to consider is that of a much more violent gust, but according to the best authorities on this subject, this is unlikely to have existed under the given weather conditions. In fact, an uprush of air of from 8 to 10 meters per second is generally considered extreme ("The Strength of Rigid Airships," Journ. Royal Aer. Soc., 1923, p. 383).

Consider next *additional stresses due to gas pressures.* When the ship broke, she was at an altitude of about 3500 feet, which was much below pressure height, since gas had been valved for five minutes during the second rise and the pressure exerted by the gas was only that due to its buoyancy. In a calculation submitted by Mr. Burgess, it is assumed that just before the ship broke the cell No. 13 at frame 120-130 was about 90 per cent full. With a lift of .0545 pounds per cubic foot, the gas pressure at the top of the cell would be 3.54 pounds per square foot, which would produce a compressive stress in the apex channel of the C-longitudinal of 3190 pounds per square inch. At the same time the rise of the intermediate frame 125 would cause an additional stress of 4090 pounds per square inch, or a total of 7280 pounds per square inch at the joints. Adding this to 13,200 pounds per square inch caused by the sagging moment, we obtain a total of 20,480 pounds per square inch.

Experiments with strain gauges have shown that the stresses are irregularly distributed among the girders, and it is found that the stress in any one girder may be 25 per cent greater than calculated. Adding 25 per cent to the total calculated stress in the C-longitudinal, we have a stress of 25,600 pounds per square inch, which is very close to the failing stress of 26,000 to 27,000 pounds per square inch at the joint where the latticing is double.

At a point on the longitudinal 26 inches from the transverse frames where the double latticing ends and the latticing becomes single, the crippling stress is only 20,000 pounds per square inch, and provided we allow again an addition of 25 per cent to the calculated stress for this point, we shall find that the critical stress may well have been passed.

It is thus possible to account for the breakdown, assuming an exceptionally violent gust and taking into account the stresses due to gas pressures which may reasonably be supposed to have existed.

If cells Nos. 9, 10, 11 and perhaps No. 12 were relatively more deflated than the cells forward and aft, an extra sagging moment would exist, which, together with the effect of the gust, would amply explain the breaking of the ship.

THE GAS VALVES.

The valve arrangement with which the ship was equipped during her last flight comprised eight automatic valves fitted near the bottom of the cells and eight hand valves fitted at the top. It was installed in the overhaul period, June, 1925, and represented a radical modification of the original design, which comprised nineteen automatic valves, one in each cell except No. 20, and sixteen hand valves. While the latter arrangement permitted a maximum rate of ascent of about 800 feet per minute without the use of hand valves, the arrangement installed on the last cruise permitted a limiting rate of only about 400 feet per minute. If the rise was more rapid, it soon became necessary to open the hand valves. Zeppelin practice was to provide for a rate of ascent of at least 600 feet per minute, but 1000 feet per minute was preferred.

All the testimonies go to show that at no time was there excessive gas pressure in any of the cells during the rise to maximum altitude (second rise). Apparently the use of hand valves for five minutes did relieve the pressures and calculation indicates that when the ship was at the peak, the pressure at the top of the cells was only 8 pounds per square foot. It is quite consistent with this condition that there was a considerable deflation of the cells during the subsequent descent of some 3000 feet. Collier saw cell No. 9, and the next cells as far forward as he could see, deflated considerably just before the ship broke, when the ship must have been at about 3500 feet altitude. At the same time, however, he noticed that cell No. 8 was up to full pressure and if such was actually the case, it would indicate that *the automatic valve* in that cell had not functioned in the same way as in cell No. 9, which was deflated to about 70 per cent of its full

capacity. This seems to show that certain valves did not function as they should, but it does not prove that excessive pressure existed at any time.

As regards the *functioning of the hand valves,* it is of interest to note Captain Heinen's statement before the Court that the deformations of the hull under severe conditions of strain may cause some of the operating wires to the hand valves to slacken or to tauten relative to the others. In such a case a collective operation of these wires by means of a single reel would not give an uniform opening of the valves. Some would open more than others. Captain Heinen ascribed this effect to a deformation of the transverse frames. The valves are adjusted in the hangar under normal weight and lift conditions when the ship is at ground level. Now, when the ship goes above pressure height, the frame will tend to approach the circular form, bulging out at mid-height, and thus the A-girder will approach the top of the keel, but this deformation will be much greater at midships than at the ends and thus a relative slack will be produced in the wires of the midship valves and the valves if operated by a common reel will not open by an equal amount. It is found that this cause may well account for a slack in the wires amidships of about one inch. When the ship is falling from a height above pressure height, these conditions are reversed.

The slackening of the valve lines in the middle portion of the ship may be augmented by the longitudinal deflection of the ship. Suppose, for instance, that the ship is subject to a sagging moment, which causes an average tensile stress in the keel of 2500 pounds per square inch, we shall then have a tautening of the wires from the control car to the after hand valve, while no corresponding tautening takes place in the wires to the valves near the control car. The distance from the control car to frame 40 is about 400 feet equals 4800 inches. Take the modulus of elasticity of the duralumin structure to be 8,000,000 pounds per square inch, allowing for a small slip in the joints, we have then a tautening in the wire leading to the hand valve of cell No. 4 of:

$$\frac{2500 \times 4800}{8{,}000{,}000} = 1.5 \text{ inches.}$$

If, now, the reel, which operates the wires, is turned, it will open the after valve fully, but the valves forward of amidships, and in particular those of cells No. 14 and No. 15, will be only partly opened. Under a hogging moment these conditions are reversed.

Only experiments under actual service conditions can furnish accurate information as to the quantitative values of these effects, but until such information is at hand it seems dangerous to rely on hand valves for the relief of pressures, especially as these valves must be located in positions where they can only be inspected with difficulty. It has been calculated by Mr. Burgess that if the hand valves near the middle of the ship opened only 50 per cent during the second rise, a critical stress would come to exist at the highest altitude, and any less opening of the valves would probably cause disaster. Hand valves, moreover, suffer from the defect that their efficiency and readiness for service cannot be tested habitually without a great loss of gas. The fact that the hand valves were used timely and with good judgment in the *Shenandoah* does not offer any guarantee that they will always be so used.

It seems safer to rely primarily on automatic valves. If every cell is provided with an automatic valve, we avoid the complete dependence on the human element, and the hand valves need be used only in cases of extreme emergency.

The use of jam-pot covers in flight seems objectionable. The fact that they were removed in time on board the *Shenandoah* does not prove that they will always be so removed. In spite of the advantage which this appliance may have in preventing the pollution of the helium, it seems better to attain the desired result by other means and it is recommended that research be made in this direction.

POWER PLANT.

It is not clear that higher power and hence higher speed would have saved the *Shenandoah*. Undoubtedly, had her speed been higher she might not have run into the particular disturbance where she was actually wrecked, but it would not necessarily have prevented her from running into another disturbance. In this respect the courses steered by the ship were of far more impor-

tance than the speed. It cannot be said, therefore, that the removal of engine No. 6 had any bearing on the loss of the ship.

Apart from this particular accident, when the ship was caught in a thunderstorm area, it is clearly desirable that airships should be capable of going at high speed, so as to make them, as far as possible, independent of the air currents, but it must also be considered that an increase in engine power involves an addition to the load carried by the ship and it becomes a question whether the same weight cannot be otherwise more advantageously employed. In the *Shenandoah* the weight, which became available by removing power car No. 6, was utilized by fitting a more powerful radio set, thereby increasing the value of the ship as a scout. Also a radio compass was installed, so as to aid in determining the position of the ship. The removal of the engine and therewith of the propeller, rendered it possible eventually to raise the control car close up under the keel, a change which was contemplated, but was not yet carried out when the ship was lost. Had this been done, it would probably have resulted in the saving of a number of lives, since the control car would not then have fallen away from the ship. In that case the after body of the ship might not have parted from the forebody. The latter would not have been so buoyant, the tendency to part would have been less and the control cables might have been capable of holding the ship together. Possibly the whole ship, although in a broken condition, might have made a landing before complete rupture occurred.

There must always be a limit beyond which it is not profitable to increase the speed, that is, when it cannot be done without undue detriment to other qualities, such as radius of action, strength, general equipment and useful load. The problem of attaining the most harmonious result by giving to each of the principal features of the design its proper weight, is particularly difficult in airships — more so than in surface vessels — on account of the extreme requirements to lightness and the limited experience in the service and navigation of such craft. It is not a purely technical problem, as it is bound up with navigational and strategical conditions. It cannot be settled by the technical designers alone, but requires for its satisfactory solution a joint study of several alternative designs by the technical and the operating personnel.

In any case, it seems inadvisable to give an airship an extra engine to be used only in case of emergency. To sacrifice a great part of the precious lift for the purpose of carrying what will be ordinarily a dead weight can only be justified if the safety of the ship is thereby materially increased. But, from the point of view of safety, it appears that an increase in radius of action by carrying a greater amount of fuel is of more value, and reserve fuel has the advantage that it may be used in emergency as detachable ballast.

THE DURALUMIN STRUCTURE.

[This chapter of Professor Hovgaard's argument was largely an abstract of the Bureau of Standards report and of Dr. Tuckerman's testimony. It is therefore omitted here. Ed.]

SUMMARY.

1. It is established that various breakdowns — probably snapping of wires — occurred during the second rise, shortly before the ship attained its highest altitude, and also during the subsequent rapid descent. It is not known whether these breakdowns had any appreciable influence on the subsequent breaking of the ship.

2. It appears that the ship broke primarily due to an excessive sagging moment caused by a violent upward gust during the third rise, but other contributive causes probably existed.

3. The breakdown is believed to have commenced with a collapse of the longitudinal girders in the upper port quadrant of the structure in the frame space 120-130.

4. The loss of the control car was induced by the breakage of the ship. The middle and after portion was heavy due to loss of gas and tended to tear away from the forward portion and in so doing a strong pull was exerted on the control cables and hence, after the sheaves had carried away, on the car. The car suspensions gave way, the car swung down and rolled over to port, the control cables parted, the car tore away and fell to the ground.

5. When the control cables became free and the control car fell, the ship separated into two parts at frame 125. In fact, it appears, that from the time when the break began until the con-

trol car fell, the control cables held the ship together and it is possible that if these cables had not parted the ship, in spite of her broken condition, might have made a fairly successful landing and the loss of life might have been much smaller. The middle portion, from frame 110 to 125, broke away from the tail, but it is not known whether this occurred at the same time as the breaking at 125 or later.

6. The loss of the control car and the swinging down of power car No. 4 were due primarily to their detached suspended position. This mode of suspension was the immediate cause of the major loss of life.

7. There is fairly reliable and corroborated evidence that the gas valves did not function uniformly. Just before the ship commenced to break, it appears that cells Nos. 9, 10 and perhaps 11 and 12 were considerably deflated, perhaps as much as to 70 per cent capacity, while No. 8 was up to pressure. As cells Nos. 8 and 9 had an automatic valve each and no hand valves, they should be in the same condition, if the valves had functioned satisfactorily.

8. There is no evidence to show that the pressure was excessive in any of the cells at any time, but if a relatively great deflation actually existed in the cells Nos. 9, 10, 11 and perhaps No. 12, while the cells beyond this region at either end were completely or 90 per cent full, a great sagging moment would be created, which might well have contributed to the breaking of the ship.

9. There is nothing to indicate that the removal of engine No. 6 had any bearing on the loss of the ship. In fact, it does not appear that the loss was due to lack of power and speed.

10. There is no evidence to show that the changes in the valve system, which finally resulted in the adoption of eight automatic and eight hand valves, contributed directly or indirectly to the loss of the ship. A timely and effective use of the hand valves prevented the building up of excessive gas pressures. Yet this system is inherently dangerous due to the fact that it makes the control of the pressure in the bags depend essentially on the human element. The non-uniform functioning of the valves, referred to under (7) above, might occur whatever the system of

valves and might have happened equally well with the original valve system.

11. Numerous specimens of the duralumin structure taken from the wreck were carefully examined and tested by the Bureau of Standards. It was found that intercrystalline corrosion and surface pitting occurred sporadically over the entire ship, but nowhere of serious depth or extent. The deterioration did not weaken the ship and could in no way have contributed to its loss. There was no sign of fatigue stresses or of local corrosion due to spillage or neglect.

12. Nothing has come to light to indicate that the structure of the *Shenandoah* suffered from overstrains incurred in previous service, when she started on her final cruise.

13. A careful investigation of the *Shenandoah's* design was made at the time of her construction by an independent Special Committee of technical experts appointed by the National Advisory Committee for Aeronautics. This committee found that the design was carried out on the basis of a thorough study by the engineers in charge, who showed good judgment in applying their results and great care in executing the plans.

14. The modifications made in the design of the *Shenandoah* as compared with the German dirigible *L-49*, selected as the prototype, were based on sound considerations, and added materially to the strength of the new ship. Every addition to the weight of the structure meant an addition to the strength.

15. As seen from the first three paragraphs above, the cause of the breaking of the structure of the *Shenandoah* has not been definitely established; it has only been possible to present and discuss various possible causes, assigning to each its probable weight. The actual process of breaking up occupied less than one minute, during which time no one in the ship was in a position to observe continuously the behavior of the structure as a whole. In general, no one could see more than what happened in his nearest vicinity. An estimate of time intervals and of the sequence of events in such a catastrophe is for psychological reasons extremely difficult; yet it is a remarkable fact that the testimonies in the *Shenandoah* case are with few exceptions very consistent in these respects. Our failure to determine the causes of the breakdown with cer-

tainty and in a complete manner is due not so much to a lack of knowledge of the events as they happened on board the ship as to the complex nature of the structure and our ignorance of the invisible and varying aerodynamic forces to which the ship was subject. Under such conditions a vigorous analysis is impossible.

RECOMMENDATIONS FOR FUTURE DEVELOPMENT.

1. It is recommended to adopt a smaller fineness ratio; length to diameter, than used in the *Shenandoah*.

This ratio is 8.6 in the *Shenandoah* and 7.25 in the *Los Angeles*, but wind-tunnel tests have shown that forms of a much smaller ratio can be produced having much less resistance to propulsion. Even a ratio as low as 4½ can be adopted with advantage as far as resistance is concerned. (Technical Report, Nat. Adv. Comm. for Aeronautics, 1918-19, p. 242). It has been found that the insertion of a cylindrical portion is unfavorable to resistance.

Ships of relatively short length and great diameter are less susceptible to the effect of gusts. The bending moments produced by aerodynamic forces may not be much smaller than in long, slender ships, but the resistance of ships to withstand bending moments and shear forces increases materially with the diameter. On the whole, short, full ships are superior to long, slender ships of the same volume, in their power to resist aerodynamic forces.

In ships of short length the static bending moments can readily be made smaller than in long and slender ships.

The principal objection to a reduction of the fineness ratio in rigid airships is the increase in weight of the transverse frame system but possibly this difficulty can be overcome by a change in the design of the transverse frames. Also the requirements to steadiness of steering may prescribe a fineness ratio greater than that determined with sole regard to resistance to propulsion and strength.

2. The control car should be built in one with the keel structure and placed close up, against the under side of the keel, instead of being suspended a distance below the ship.

The arrangement here proposed is adopted in the *Los Angeles* and is believed to have been contemplated in the *Shenandoah*. It facilitates more secure attachment of the car to the ship and will

prevent the car from tearing away as occurred in the *Shenandoah* with such disastrous results.

By placing the control car contiguous to the ship the air resistance can be reduced.

3. It is recommended to investigate the possibility of placing the engines inside the ship instead of in detached power cars and of using only two propellers placed aft as in a submarine.

The power cars hitherto used as open to the same objections as stated above for the control car; danger of detachment and increased air resistance, and they also furnish rather unfavorable conditions for working of the propellers. The propellers are placed close behind the cars, preventing a free flow of air; the slip streams of the forward propellers are apt to disturb the action of the after propellers, and the proximity of the hull to the propellers causes augmented resistance due to surface friction.

With a full form of the ship as here proposed it should not be difficult with only two propeller shafts to house the engines in the structure either in the keel or adjacent to the keel on each side, leaving the form of the ship perfectly fair. Each shaft could be driven by two or three engines, so placed longitudinally as to distribute the load properly; transmission gear to be used as found necessary. The principal objection to this arrangement appears to be the great length of shafting required and consequent liability to disturbance of the alignment due to deflections of the ship, but this difficulty can probably be overcome by means of flexible couplings. Also the weight of the shafting may prove to be excessive, but may perhaps be reduced sufficiently by giving a great speed to the shafts.

4. It is desirable to increase the speed of airships so as to render them more independent of the air currents.

By adopting a more full form and the arrangement of machinery proposed above, involving the abandonment of suspended cars, a higher speed can be attained on a given power without any sacrifice in strength or fuel capacity.

Higher speed can also be attained by increasing the size of airships, and may in such case be associated with increased radius of action.

5. It is recommended to undertake research with the object of determining whether duralumin girders of greater strength against crippling can be designed without an increase in weight.

The design of girder hitherto used fails in compression by flexure with the elastic limit of the material long before the tensile strength is exhausted. It may be possible to produce girders that will stand a higher unit compressive stress on a given total weight, but this problem can only be solved through study and experimentation.

6. It is recommended to adopt an arrangement of automatic and hand valves essentially as in the original outfit of the *Shenandoah*.

By fitting an automatic valve in practically every cell, the dependence on the human element in regulating the gas pressure is avoided.

7. It is recommended to build ships of moderately larger size, say, five million cubic feet displacement for war service and for long distance commercial travel. Such ships can be given greater speed and cruising radius.

While the design of a large type ship is under preparation it is highly desirable that a smaller ship should be available for research work and for training of the personnel. The maintenance of a skilled and experienced personnel is a fundamental condition for success and can only be obtained through continuous service over long peroids.

It is therefore recommended that a ship of about one million cubic feet displacement be built at the earliest possible date, and that its construction be accelerated as much as practicable.

8. Additional mooring masts and other necessary equipment for handling airships should be installed at proper locations within a wider and wider field of operation.

9. A weather service specially adapted as an aid to air navigation should be established within the intended field of operation.

10. In the further development and operation of airships a systematic policy should be followed.

In the beginning, research work, scientific tests and the requirements of designers and constructors should be considered of primary importance.

Next to the technical requirements, greatest weight should be given to the systematic training of the personnel. Special efforts should be made to preserve the continuity in the service of the personnel in each particular type of ship, rigid, semi-rigid or non-rigid, as also in the service of construction and design.

Gradually, as the material and personnel is perfected, strictly military or naval duties can be taken up.

The area of operation should be systematically and cautiously extended, beginning with restricted areas or zones, where adequate mooring facilities and a satisfactory weather service are established. Gradually as experience is gained and the said facilities can be provided over larger areas, the field of operation can be extended.

It is to be realized that airship design, construction and navigation are new fields of human endeavor, comprising many difficulties and dangers, which are as yet incompletely known and understood. The numerous disasters which have so far accompanied the development are not necessarily inherent in the art, but follow from a failure to realize the magnitude of the difficulties that surround the problem.

The best policy seems to be a vigorous research and development work and a careful, systematic training of the personnel, followed by cautious progress in operation as allowed by a gradual extension of the accessory services.

BRIEF SUBMITTED BY INTERESTED PARTIES (SURVIVORS) IN THE CASE OF THE "SHENANDOAH" COURT OF INQUIRY.

Although the facts developed before this court in connection with the loss of the *Shenandoah* have been clearly brought forth and freed of any original doubt or uncertainty, we nevertheless have deemed it advisable to prepare this rather extensive summary in order to effect our own analysis of this case more proficiently, and to assist the Court as may be possible by submitting the case in the light that it appears to us.

Attendance throughout the sessions of this Court and a study of the record of the testimony and of the evidence submitted show decidedly on the part of each and every witness the desire to tell fully and of every pertinent incident within his knowledge on the

subject of the loss of the *Shenandoah*. The survivors who have been designated as "Interested Parties" have had as their motive in their direct and cross-examination of witnesses that of assisting in the inquiry by developing to the best of their ability fully and clearly many points peculiar to the *Shenandoah* or to airships and their operation with which it is not fair to expect that anyone could be conversant without an intimate association or only after a great deal of study of such subject matter.

To determine the comparative weight to be given the testimony of each witness it is necessary to balance such testimony against the source of his knowledge, his apparent accuracy, his technical training, his experience and his particular expertness. The testimony of witnesses that is based on hearsay or second-hand information necessarily cannot be of great value regardless of the proficiency or expertness of such witnesses in other or allied subjects. Furthermore, there have appeared before this Court witnesses who, even if they had been supplied with complete and accurate information of the facts and circumstances surrounding the loss of the *Shenandoah*, have shown us by their testimony that they did not possess the training, experience or knowledge of the subject sufficient to form an intelligent opinion. Then, again, from our intimate knowledge of the *Shenandoah* and her personnel, it has been apparent that a few witnesses among the survivors have made statements purported to be of their own definite knowledge relative to observations during the disaster that manifestly were impossible or at least highly improbable of accomplishment on the part of those particular witnesses. Such statements were not, however, made with the slightest thought of improper intent, but were doubtless colored by these witnesses having heard the testimony of numerous others. This and the desire to appear to have been intensely observant under the tenseness of the disaster are but human failings, but they impose on the Court the additional duty of determining credibility. However, we believe that the few such cases were self-evident and only in exceptional cases will time and space be taken herein to point them out.

From the direct examination of Colonel William Mitchell it was very apparent to us that this witness had no expert knowledge of his own relative to the *Shenandoah* disaster, and any further

examination of the witness to adduce testimony expert or otherwise or information of any value on this subject would have been unnecessary as well as futile. As a matter of fact, the direct examination of this witness substantiated fully, an utterance of the defense counsel, the Honorable Mr. Reid, made before the court-martial recently engaged in the trial of Colonel Mitchell. Although apparently intended for but one of Colonel Mitchell's statements, we feel that as far as this Court of Inquiry is concerned, any and all statements that Colonel Mitchell could possibly make on the subject of the *Shenandoah* disaster would be of exactly the same nature. I read from an article appearing in the press on 28 October, 1925, wherein his counsel stated: "Any one who read Colonel Mitchell's statement would recognize that it is a fine play of words that really means nothing."

Major Frank Kennedy, U. S. Army, who appeared as a witness, offered a statement in evidence that he had seen in the after portion of the *Shenandoah* wreck four filling sleeves of gas cells tied off. Although present at the scene of the wreck admittedly for the purpose of being of assistance and at various times in conversation with surviving naval officers and members of the Board of Investigation, his intuition or whatever it may be styled, failed to cause him to point out such a salient feature to any naval authority capable of proper cognizance of this feature. To us, it is strange that such an officer, supposedly of considerable airship experience, could or would restrain himself in the divulgence of such admittedly important evidence until the opportunity occurred to present it in a derogatory light and then to the *proper* Court only after the process of subpoena.

To point out the absurdity of the testimony of this witness it is necessary to cite only one example. When asked to point out in Exhibit 94 (submitted by himself) if there appeared therein within the limits he had described, one of the filling sleeves to which he had referred, although he was positive as to his original allegation concerning the tie-off, the witness replied that he did not know that there was *not* one of these filling sleeves included in his marked-off limits. Startling as it may seem, a competent witness put on the stand immediately thereafter identified positively in Exhibit 94 one of the very sleeves, completely open and free

from tie-offs, that Major Kennedy had shortly before failed to recognize. As established by competent witnesses, the tape "tie-offs" to which Major Kennedy referred were the means of suspending the manifold throughout the *Shenandoah's* keel. In our opinion, the testimony of Major Kennedy added nothing whatsoever and served only to necessitate valuable time to discredit a groundless fancy and to clutter up the record of this Court.

In our opinion an analysis of the testimony of Captain C. P. Clark, U.S.A., shows that he failed to qualify as an expert on any matter pertaining to rigid airships. Although it is understood that he testified in a positive manner before Mitchell court on matters pertaining to the *Shenandoah* disaster, his examination before this Court revealed nothing whatsoever except his own valueless groundless opinions, and his testimony may be dropped entirely from consideration.

The testimony of 1st Lieut. Orville A. Anderson, U.S.A., was frank and above-board. He did not put forth his opinions as anything but opinions and his testimony in general is corroborative in many points of the contentions of the survivors.

The most recent testimony of J. H. Collier, Aviation Chief Rigger, a survivor of the *Shenandoah*, in answer to a question, shows that he did not experience any rapid rise of the *Shenandoah* or any rapid fall just prior to the breaking up of the ship. It is, therefore, very easily understood that Collier did not awaken until after the ship had broken in two and that any cell or cells that he saw in a ruptured condition were the *result* of the breaking of the ship rather than of its *cause*.

The testimony of Captain Anton Heinen, being of a very positive nature, requires on certain points equally positive refutation. While there is no disputing of the fact that Captain Heinen is an expert *operator* of rigid airships, we have in the record his own statements to the fact that he is not an engineering expert and that his own knowledge on such engineering matters pertaining to airships is gained entirely by superficial observation rather than by an intimate study of such subjects. It is very apparent that a great many of his statements made early after the *Shenandoah* disaster and even before this Court, were made in the heat of passion and had for their object that of sustaining certain theories

that occurred to him and from which not even the cold facts could cause him to recede. Knowing Captain Heinen's excellent record of cool performance in flight, we regret that it was not possible for him to have made his pronouncements on the *Shenandoah* disaster from a considerable soothing altitude rather than to have been fired by his contact with the press and with the earth. It is likewise equally to be regretted that such an expert, knowing the ramifications of airship operation should lay himself open by making the most positive affirmations based solely on press reports, while he himself was several hundred miles from the disaster and when only the most salient features of the cause of the disaster had been made known.

The survivors wish to take this opportunity of expressing their appreciation of the very valuable and elucidating testimony given before this Court by a number of expert witnesses including: Professor C. P. Burgess, Dr. W. J. Humphreys, Dr. W. R. Gregg, Professor Alexander McCadie, Dr. Earl Blough, and Dr. L. B. Tuckerman. The character and experience of these witnesses and the clear lucid nature of their extensive testimony makes such testimony of great fundamental value not only for the purposes of this Court, but as well to all who pursue the subject of rigid airships in any of its ramifications.

The *Shenandoah's* Mid-west Flight was made in compliance with full and legal orders issued by the Chief of Naval Operations in his despatch of 11 June, 1925, and the subsequent letter and despatch modifications thereof as they appear in the record of this Court.

There has been brought out in the testimony received before this Court an attempt to make it appear that the Commanding Officer of the *Shenandoah* undertook this Mid-west Flight under protest. It is the belief of the interested parties (survivors) that such contention is not in accordance with the actual facts.

From statements widely circulated in the press, there is evidence of the prevalence of opinion that Lieutenant Commander Lansdowne undertook the Mid-west Flight of the *Shenandoah* under protest because he feared the weather at that time and that he started the flight at the scheduled time merely because he considered it his duty to carry out his orders without the appearance

of quibbling, of insubordination, or of the stigma of cowardice. In our opinion an improper construction of his attitude towards the Mid-west Flight might easily, by imputation or inference, be cast upon the late Lieutenant Commander Lansdowne, a stigma that is not only unwarranted but libelous.

The resumé of the official correspondence, now a part of the record of this Court, shows unmistakably that Lieutenant Commander Lansdowne at no time protested the Mid-west Flight as ordered for the first week in September, but did express a preference for the second week of September on the grounds of thus providing additional time for preparation of landing facilities along the proposed route. When it was evident that the ground-handling facilities would be in order by the first week in September the estimate of the situation in the mind of the Commanding Officer of the *Shenandoah* was changed as is indicated by his acquiescence in his letters to the Department.

That the Commanding Officer of the *Shenandoah* had no definite or concrete fear of the weather even in the latter part of August is evidenced strongly and definitely by his recommendation for a special trip to Detroit in the last week of August, the route of which would have exposed the ship to the same geographical conditions as did the first and last legs of the Mid-West trip as finally ordered and acquiesced in.

There has been introduced before this Court not one bit of evidence even tending to show that Lieutenant Commander Lansdowne protested the flight because of his fear of the weather in the early part of September. Furthermore, it is evident that the objections raised by the Commanding Officer of the *Shenandoah* in June regarding unfavorable weather and handling facilities no longer bore weight for the trip as ordered for September.

If by inference, insinuation or otherwise it could be established that Lieutenant Commander Lansdowne actually feared the weather of the first week of September, and that fearing such he failed to make proper representation of such apprehension, the logical conclusion that inevitably must be reached is that Lieutenant Commander Lansdowne, in view of the elasticity of his orders, wilfully and knowingly hazarded his ship and jeopardized the lives of the forty-two others of his crew by starting at the

time he did. Yet here we had the example of an officer who was enjoying the deserved admiration of the world for his airship exploits; who had been selected because of his ability and reliability to command the *Shenandoah* on the Proposed North Polar Expedition; who occupied the highest position in the airship world then possible for him to attain; who was now embarking on a flight of importance that he expected to be his last before beginning another detail that would enable him to receive promotion. To believe, then, as one must should he be convinced of the alleged reluctance of Lieutenant Commander Lansdowne to carry out the flight as ordered, that such an officer would jeopardize his crew and his ship wilfully and knowingly, not only approaches the height of absurdity but slanders the dead.

* * *

STRUCTURAL INTEGRITY OF "SHENANDOAH."

One of the most necessary and logical phases of the investigation of the loss of any vessel is that of looking into the structural integrity and material condition of the ship in question. Testimony before this Court has pointed out that the *Shenandoah* was ready for flight eleven days prior to the actual beginning thereof. During this period, the crew was occupied with work of inspection and of a nature of perfecting every detail of the readiness of the ship to perform with entire success such an extended flight. As an example of the meticulous care with which these preparations were conducted, it is pointed out that two gas cells, the purity of whose gas under ordinary operating conditions was satisfactory, were deflated and reinflated with helium of slightly higher purity.

The structural integrity of the *Shenandoah* was unquestionably excellent. During the period from November, 1924, until June, 1925, while the *Shenandoah* was deflated and consequently in an inoperative status, continuous and repeated inspections of the entire structure could be made easily and were made. Such inspections at no time disclosed any signs of faulty material. This fact is borne out consistently by the testimony of those members of the *Shenandoah* crew who were charged with such upkeep duty as well as by witnesses of an expert character, such as Dr. Earl

Blough and Commander Weyerbacher, U. S. Navy. Both of these witnesses who had made inspections of the *Shenandoah* at times prior to her last flight, as well as immediately after her destruction, state that in not a single instance in their examination of the material of the *Shenandoah* was there any sign of undue structural failure, of deterioration, or of faulty material.

Dr. Blough's testimony shows that from his inspection of the wreck of the *Shenandoah* very shortly after its occurrence the *Shenandoah's* structural material was remarkable for its consistent cleanliness. Although he expected to find a few minor defects, actually none were found and the examination of even hundreds of fractures revealed absolutely nothing to indicate defective material.

Water recovery units had been installed on the *Shenandoah* after her commissioning for the purpose of conserving helium. At the time of her disaster four engines were equipped with such water recovery units, one of which was a new two-stage apparatus. These units were additions to the airship and in no way did they affect the integrity of the structure, in that these installations in no way involved any weakening or removal of structural members of the ship. Indirectly the water recovery installation increased the structural factor of safety of the airship as it prevented the attainment of that "light" static condition in which the static stresses would be the highest. Further, in preserving the balance by the lift and load of the airship, the water recovery installation reduced the dynamic stresses which would be caused by flying out of trim. Summed up, it is a fact that the water recovery installation of the *Shenandoah* could not have had anything but a favorable effect on the structural strength of the ship.

Records of the Bureau of Aeronautics show that upon the completion of the *Shenandoah* her actual returned weight was within 1 per cent of the weight estimated by her designers. After the breakaway of the *Shenandoah* from the Lakehurst Mooring Mast in January, 1924, the structure was repaired and as brought out by witnesses was actually strengthened in certain localities above the designed strength. Such strengthening, of course, involved a slight amount of additional weight. The added weight, which was actually 1500 pounds, represents an increase of about 5 per cent

over the original weight of the structure itself and only 1.7 per cent of the total dead weight of the *Shenandoah*.

The records of the *Shenandoah* show that the ship had cruised during her existence a total of 740 hours actually underway, for a total distance of about 25,835 miles. Of this, 605.5 hours of flight, covering 20,671 (Ground) miles were subsequent to the repair period of the ship following her breakaway from the Lakehurst Mooring Mast on 16 January, 1924. In other words, the *Shenandoah* performed about five times the amount of cruising after her breakaway that she did before it. This we consider to be the positive proof of her structural fitness.

SUBSEQUENCE OF EVENTS IN LOSS OF "SHENANDOAH."

Following is the sequence of events leading to and during the *Shenandoah* disaster as they appear to us from the evidence adduced:

(*a*) The *Shenandoah* was caught in a forming atmospheric disturbance unmarked by visual indication until after it had enveloped the ship and this visible only from the ground;

(*b*) Immediately that the ship was known to be in the disturbance as detected by her rapid rise, every possible effort was made to counteract the rise and to prepare for going over pressure height;

(*c*) Jam pot covers were removed from all automatic valves; the gassing manifold and inflation sleeves to gas cells were unstopped;

(*d*) At or about 3800 feet (*i.e.*, just below pressure height), the rapid rise was checked for a few minutes. This checking was only temporary as the ship began to rise rapidly again, this rise being accompanied by continued and increasing rolling and pitching;

(*e*) During this phase of the rise over pressure height the maneuvering valves were opened for five minutes to assist the automatic valves in their discharge of helium and thus prevent any over-pressure;

(*f*) Shortly after ceasing to valve gas, water ballast was dropped to counteract the "heaviness" acquired by loss of buoyancy through valving;

(*g*) At or about 6200 feet (by altimeter) the ship encountered a descending current (or got out of the ascending current) and began descending at a terrific rate. Additional water ballast was dropped and the violent motion of the ship increased;

(*h*) At about 3000 feet, the ship encountered another rising current and reversed in a very brief space of time from a falling body to a rising one. This change was accompanied by a spinning of the ship around a vertical axis in a counter clockwise direction and by aggravated rolling;

(*i*) It is believed that the initial failure in the structure occurred with this reversal from falling to rising, the failure occurring in the top of the structure just forward of frame 120 on the port side. The failure of the structure then progressed rapidly causing the deflation of cells Nos. 9, 10, 11, 12 and 13. The collapse of these cells led quickly to the rapid disintegration of the structure in their vicinity. The stern having been at an upward angle and the bow being *hurled* upward, caused the ship to open up on the bottom. This completely separated the ship into two parts, the break being between frames Nos. 120 and 130.

(*j*) The stern section, deprived of the lift lost by the deflated cells and carrying all the power cars, crashed at once.

(*k*) At the opening up on the bottom, the major control cables (Rudder) not being able to stretch to accommodate this parting of the structure, caused the collapse of the control car strut system by compression. The control cable sprocket chains, jamming in the control sheaves, tore out this control sheave joint immediately above the control car. This joint was also a support for the starboard strut connection. Having lost the starboard suspension system the control car swung freely and the rapid accelerations and decelerations of the car caused almost immediately the collapse of the entire system, precipitating the car to the ground.

(*l*) Relieved of the control car and the after part of the ship, the forward section rose and free ballooned to a safe landing.

(*m*) Either upon crashing or very shortly before, the section from about frames 90 to 130 left the stern and came to rest.

(*n*) The swinging of No. 4 power car from its original position is believed to have been a *result* of the parting of the structure rather than a contributory factor to the latter.

WEATHER CONSIDERATIONS.

The testimony of the survivors and of the meteorological experts brought before this Court has clearly established the following facts relative to the weather conditions prior to and during the last flight of the *Shenandoah:*

(*a*) That the storm which caused the disaster was associated with a cyclonic storm of the quite ordinary summer type, but with an unusual direction of travel.

(*b*) That there was nothing on the weather map of September 1st or 2nd to warrant or even suggest the cancellation or postponement of the *Shenandoah's* Mid-west flight.

(*c*) That the southwesterly winds encountered by the *Shenandoah* 1500 feet above the ground (2500 feet above sea level as indicated by the altimeter) during the night, were in accordance with the weather maps of both the morning and the evening of September 2nd and of the morning of September 3rd.

(*d*) That the winds 1000 feet above the ground were doubtless more southerly than at 1500 feet since such conditions are normally so in the southeast quadrant of a cyclonic storm.

(*e*) That the drift of the ship to the right at all times during the night except for the short period when the ship was headed toward the south, was normal and had been noticed as was the fact that the angles of drift to the right increased as the strength of the wind increased and also as the ship was brought from higher to lower levels whereby the southerly component, due to the greater wind, became greater.

(*f*) That the changing of the drift from left to right occurred as the ship changed course from south to west or westsouthwest and was normal and in no way constituted a danger sign.

(*g*) That a cold air current had invaded Indiana and Ohio at an altitude of about 7000 feet and above, over-running the warm southwesterly winds below, thereby setting up a very unstable condition.

(*h*) That such unstable conditions will ultimately cause vertical currents often of considerable velocity and will produce cumulus clouds and thunderstorms if there is sufficient moisture content present.

(*i*) That the cumulus clouds are the *result* of these air currents and not the cause and that, therefore, the rising currents may be well established before clouds develop.

(*j*) That without cumulus clouds, flying particles of smoke or dust, there is no way at present of determining the presence of such vertical currents from a distance.

(*k*) That vertical currents can be and usually are so sharply defined that the air at a distance of 500 to 700 feet is undisturbed.

(*l*) That the best indication the captain of an airship has that dangerous vertical currents exist or are forming, is the presence of cumulus clouds. As a matter of fact, the drift of the ship has nothing to do with this matter.

(*m*) That unstable conditions as described, namely, the cold air at and above 7000 feet and the warm air below, could very easily be established immediately *above* an airship or above a person on the ground without such conditions in any way being suspected and that this condition could then cause the development of clouds, squalls and thunderstorms over considerable areas over a very short period of time.

(*n*) That the unstable conditions existing above Eastern Ohio on the morning of September 3rd easily account for the many storms which occurred that morning.

(*o*) That the spreading of the storm to the west would show unusual conditions and would constitute a danger sign, but that the danger at that time was quite remote.

(*p*) That a change of course at that time was not actually mandatory and would have been only an exceedingly cautious move.

(*q*) That not only the Captain and the Aerological Officer, but all the officers in the control car, some of whom were in addition to those on duty, were constantly watching for indications of the development of conditions near at hand which would threaten the safety of the ship.

(*r*) That precautions were taken for the reception of all weather information available to an airship in flight.

(*s*) That the 8 p.m. Weather Bureau signals broadcast from Arlington at 10:30 p.m., September 2nd, were received, together with the Weather Bureau Aviation Forecasts and as well also, a

special forecast from the Naval Air Station, Lakehurst, New Jersey, received about midnight.

(*t*) That a weather map was drawn on board the *Shenandoah* and a forecast prepared and that both were discussed with officers in the control car about 1 a.m., September 3rd.

(*u*) That local thunderstorms were expected to develop on Thursday, September 3rd, but a general thunderstorm condition was not expected to develop south of Lake Erie. Later events showed these expectations to be correct except for the fact that the storms developed before and not after sunrise.

(*v*) That the weather map is at present the best means for anticipating the development of thunderstorms, but if not yet formed, precise locations and times of formation cannot be determined.

(*w*) That in spite of the knowledge gained from the Weather Bureau Aviation Forecast, the study of the map on board the *Shenandoah*, the forecast from Lakehurst and the combined observations by those in the control car, the unstable conditions in the upper atmosphere spread so rapidly and suddenly and in such a manner as to entrap the *Shenandoah* in a very rapidly rising current before its presence could be discerned.

Unless carefully considered, it might be possible to derive from certain testimony, the impression that there was a difference of opinion on the part of the Aerological Officer and of others at one time during the danger period as to a change of course to southeast. Any uncertainty on this point can be dispelled by reference to the testimony of Lieutenant Anderson as previously given.

Testimony of several witnesses shows that at this time the ship was laboring heavily in the atmospheric turmoil and although the order to turn southeast was momentarily given, conditions rendered it impossible to effect this change of course and the ship would undoubtedly have been destroyed in a few moments even had the change been accomplished.

Captain Anton Heinen was asked the following question and gave the following answer on page 576 of the record:

Q. I ask you to tell the Court what in your opinion were the mistakes that were made in the handling of the ship on this occasion? A. In my opinion, the ship ran deliberately into the

center of this storm. For at least half an hour danger signs were shrieking out loud, etc.

We submit herewith brief extracts from the expert testimony of three of the leading meteorologists of the United States and of the world. One reading of this suffices to show that again the impulsive outbursts of Captain Heinen have led him to the heights of absurdity.

Professor Alexander McCadie, Dr. W. J. Humphreys, and Dr. W. R. Gregg, who appeared on the witness-stand, were each given a very hypothetical question that fully and comprehensively contained all the known data on weather, weather maps, courses, drifts and similar considerations that obtained beginning at 8 p.m., 2 September, 1925, and extending up until the *Shenandoah* disaster, over the *Shenandoah's* route. Having had this data given them, they were then questioned as follows and gave these answers:

Professor McCadie: Page 938, question 44 and answer:

Q. Would you consider the conditions in which an airship found herself above alarming? A. I take it that he was clear of the cloud screen and that the element of danger did not appear and he was justified in keeping his course to the west or southwest.

Dr. Humphreys: Page 1139, question 81, and answer:

Q. This question will cause you to recall that hypothetical question of the weather map. Having recalled those conditions as given, did you see any indication, any sign, of any immediate danger threatening the ship? A. Nothing at all, beyond the mere fact, if I understand it correctly, that the ship, although headed west, was making little speed over the ground, indicating that you were in a strong westerly wind, and at the surface there was a good wind from the southwest. * * * That would not show any immediate danger at all, so far as I can see, etc.

Dr. Gregg: Page 1160 and 1161:

Q. Are there any indications in the above observations to warn the airship of approaching danger from local squalls? A. Then I do not see in the observations stated that there was any marked evidence of danger of violent currents. * * * As long as it was not known that northwesterly wind was above the airship, I do not see that there was any evidence of immediate danger.

There were numerous other points in which Captain Heinen's testimony is at variance with that of the meteorological experts, but that just quoted is deemed sufficient to indicate to the Court that Captain Heinen's meteorological knowledge, while probably of a high order, cannot possibly be considered expert, or of value at a range of 500 miles.

VALVE INSTALLATION OF "SHENANDOAH."

The original installation of eighteen automatic and sixteen maneuvering valves on the *Shenandoah* had been in operation for a period of one year before any reduction in number was made. At no time during this period was more than a small fraction of the airship's valve capacity required, which bore out the generally accepted opinion that the German prototype on which the *Shenandoah* design was largely based, had a valve installation provided for *war*-time operations rather than for ordinary service flying. The reduction in numbers of both automatic and maneuvering valves was under consideration prior to the West Coast flight, but it was deemed prudent to make the change progressively rather than in one step, so that only eight *maneuvering* valves were removed at that time. All kinds of flying conditions were encountered on the West Coast flight and it was noted that the reduced valve capacity was still in excess of that required. Therefore, after that flight ten of the automatic valves also were removed. Flights with the final arrangement, which consisted of eight maneuvering and eight automatic valves, were made from June, 1925, to the time of destruction of the *Shenandoah*, and this installation was considered satisfactory by the operating personnel.

The reduction in the number of gas valves was recommended by the ship's operating crew primarily for the purpose of saving weight, but it was also realized that the new installation had the added advantage of reducing the infusion of air to the gas cells, with consequent reduction in rate of loss of purity of helium in the airship. In making this reduction it was recognized by the operating personnel that the inflation manifold-automatic valve combination could take care only of low rates of rise above pressure height, and that for the higher rates of rise it would be

necessary to assist by use of the maneuvering valves. This is borne out by the action of the crew in their handling of the situation during the period of danger.

It has been shown through the introduction of test data that with the final valve arrangement as existed in the *Shenandoah's* last flight, that a rate of rise of 1260 feet per minute above pressure height was possible, without endangering the safety of the ship's structure.

Never having experienced in her two years of operation a rate of rise as high as 1260 feet per minute (or 6.25 meters per second), the change in the valve installation from eighteen automatic and sixteen maneuvering to eight automatic and eight maneuvering valves cannot in itself be considered to have restricted the operation of the *Shenandoah,* nor imposed higher stresses on her structure than would have occurred with the original valve installation and original method of operation.

To decide whether the *Shenandoah's* crew carried out a proper operating practice with the modified gas cell-valve installation it is necessary to determine the following: (*a*) Were the maneuvering valves operated when necessary and were their controls properly synchronized? (*b*) were jam-pot covers off the automatic valves? and (*c*) did the omission of gas-cell manometers and gas-pressure alarm hazard or unfavorably influence the proper operation of the valve installation? These factors will be considered separately.

The survivors who were in a position to know whether the maneuvering valves were operated are in complete agreement that all such valves were opened simultaneously for a period of five minutes during the rise shortly before the destruction of the *Shenandoah.* Inspection of the valve controls was considered very important and was made after every flight by the air station inspection force, as well as by the ship's crew. The frequent checking of synchronism of the valve controls was accomplished by two independent methods: (1) while in the shed and with the ship afloat by operating the controls and watching all the valves to see that the valves were actually at the point of opening; (2) in flight, by actually valving gas and consequently determining the relative loss of gas in the various cells equipped with maneuvering valves.

In this connection it should be remembered that each maneuvering valve was provided with an opening lever so proportioned that its control wire had to move 3.45 inches for each inch that the valve lifted, so that even if small variations in control cable adjustment existed, these variations could have had but little effect on the openings of the valves. Consequently, unsupported testimony to the effect that certain maneuvering valves might not have opened, made by a witness not familiar with the method of synchronizing the maneuvering valve control, and the frequent checks and inspections made, cannot be given any weight.

Testimony of the survivors and of the officers in charge of cleaning up the wreckage at Ava and Sharon, show definitely that no jam-pot covers were on the automatic valves, so that there can be no question but that those valves were able to function properly at the time of the disaster.

The Germans did not install gas cell manometers in their airships, and the *Los Angeles* is not equipped with them. The British, however, used such manometers, but it is believed that this practice was carried over from their non-rigid airships on which manometers are absolutely essential. Manometers on gas cells give no essential operating information above that obtainable by visual examination and by the physical testing by touch of the cells or inflation manifold. The purpose of the gas pressure alarm was to notify the control car when pressure height is reached, whether purposely or inadvertently.

The *Shenandoah's* gas-pressure alarm had never functioned satisfactorily in the ship and had therefore been removed for repair a short time prior to the last flight. As a result, the keel watch was required to notify the control car whenever pressure height was being approached or reached, and this duty they had been performing satisfactorily over a period of two years even when the alarm was installed. Therefore, the omission of the gas-pressure alarm did not eliminate the pressure-height warning to the control car. Consequently, the valve installation must be considered to have been in proper operating condition and the personnel concerned fully cognizant of conditions at all times on the last flight of the *Shenandoah*.

The actual barograph record of the *Shenandoah* has had certain arbitrary corrections applied to it for the purpose of establishing the probable altitude path of the airship for a short period prior to her destruction. These corrections were consistently of the nature of large additive altitude increments. The interested parties feel that these corrected altitudes and the rates of rise derived therefrom are greater than were actually the case, but have not prepared a rebuttal of this, as they are confident that even with these worst conditions that have been assumed there still remains a sufficient factor of safety, as will be pointed out.

A hypothetical problem has therefore been worked out considering a vertical rise of the *Shenandoah* of 1260 feet above pressure height in five minutes, during which time all maneuvering valves are open. This is followed immediately by an assumed 700-foot rise in 42 seconds with the maneuvering valves closed. In such a problem it is evident that the equalization effect of the inflation manifold must be considered, also the discharge characteristics of the automatic and maneuvering valves; the temperature and pressure gradients of the atmosphere and the adiabatic cooling of the helium which occurs in a rise. Actual test data is available on the discharge characteristics of the *Shenandoah's* inflation manifold, of the maneuvering valves and of the automatic valves. These tests were made with painstaking care and under conditions that permitted far more accurate determination than could be made in an airship itself, and, therefore, they must be considered the most authoritative performance data on this equipment in existence at the present time. The *Shenandoah's* personnel had previously to those tests made an average estimate of the loss of lift of the airship with all maneuvering valves opened simultaneously, of 1 per cent per minute for landing conditions. With increased altitude valve discharge increases due to diminution of outside pressure and further it is evident that the valves in the parallel gas cells will discharge a greater volume of gas than those in the tapered cells, due to the greater pressure-head of gas in the parallel cells.

Also in landing, adiabatic heating takes place, resulting in superheating of the helium so that the loss of lift obtained indicates a smaller quantity of gas discharge than actually takes place. In a

rise adiabatic cooling takes place which counteracts to a certain extent in the lifting gas the expansive effect of the rise. Consequently the assumption that a maneuvering valve in a parallel body cell discharges helium equivalent to only one-eighth of 1 per cent of the lift of the airship with the latter rising from an altitude of 4000 to 5300 feet, is consideraby in error, even if this figure were correct for sea-level landing conditions, as assumed in testimony of Mr. C. P. Burgess.

The interested parties have submitted a solution to the hypothetical question referred to above, considering in this problem only a pair of parallel body cells, using the Lakehurst inflation manifold and automatic valve test data; the atmospheric pressure and temperature gradient given by the National Advisory Committee for Aeronautics, applying a correction for adiabatic cooling, but using the assumed low sea-level value of one-eighth of 1 per cent of the lift of ship for the maneuvering valve discharge, uncorrected for altitude. This solution of the problem involved the most unfavorable assumptions (to the ship) that have been introduced before the Court, and while the interested parties not only do not accept these assumptions, but have shown their incorrectness, still this solution proves that the modified valve arrangement as operated caused actually less pressure in the gas cells than would have occurred with the original valve installation and the original method of operation.

Summed up, it is evident that the valve installation on the *Shenandoah* was adequate for any conditions encountered and cannot be considered to have been a contributing cause to the loss of this airship.

Testimony has brought out that approximately 15 minutes before the actual breaking of the *Shenandoah* in the air that the inflation manifold was tied off between gas cells 8 and 9. The survivors who remain as interested parties do not admit that this tied-off condition remained in effect for any length of time, for other inspections made at approximately this same time showed the entire manifold open, and, furthermore, such a tying-off in going to pressure height was merely a routine function that was intended only for brief periods under certain conditions. Furthermore, the question of the recollection of time intervals on the

part of witnesses enters into this phase, but even if it is granted that this manifold tie-off between cells 8 and 9 remained until the destruction of the ship, a study of the inflation manifold and valve installation of the *Shenandoah* has shown that while such a tie-off might reduce the pressure in the aftermost cells by a slight amount, it cannot be considered to have had any unfavorable influence on the operation or on the strength of the airship. Furthermore, it has been pointed out by Mr. Burgess in his testimony that such tie-off was very probably of benefit to the ship under the given conditions rather than a source of danger.

It has been inferred in the testimony of certain witnesses that there was a possibility that the maneuvering valves did not function due to the distortion of the ship's structure or due to the breaking of a control wire. Had a control wire broken it would have been manifest at some point in its length through the keel, where the control wires were not covered and were constantly available to visual inspection. No such broken condition has been noted in the testimony of any member of the crew. Further, had a maneuvering valve control wire parted at any point, such a fact would have been known in the control car immediately upon the discontinuing of hand valving by the failure of such a broken wire to return under spring action to the proper seating point of the toggle on the valve control box. No such condition was observed and it is pointed out that this control box was in full view of every one in the control car, including some of the survivors. Reference was had in the testimony of several witnesses to the noise of the "snapping" of a wire. Attention is called to the fact that in adjusting themselves under tension, wires frequently make a "snapping" noise that resembles greatly the actual parting of a wire, but without a break actually occurring. Testimony before this Court also points out that never in the history of the ship had the *Shenandoah* experienced a broken valve control wire. The conclusive proof that the valves functioned properly is the testimony of those trained officers and men whose duty was in the keel, that at no time were there any signs of excess gas pressure. As testified to, the German practice put such faith in a man's ability to estimate gas pressure by touch that their keelsmen were instructed to slash cells if their estimate by touch called for such action.

SIXTH ENGINE OF "SHENANDOAH."

As the added weight of the new radio installation was less than the weight of the sixth (removed) engine with its transmission and propeller, this change enabled the *Shenandoah* to carry an additional amount of fuel, increasing, of course, her radius of action thereby. Further, this change permitted a better distribution of weight with a consequent reduction of the static stresses set up in the structure. Although it is agreed that in general high speed may be desirable in an airship attempting to avoid storms, it must be remembered that once within the dangerous limits of the storm area, the higher power required for such higher speed, if used, becomes a source of danger due to the additional aerodynamical stresses which might be imposed upon the structure by the operation of this engine. Although witnesses have alleged before this Court that additional power might have been of value in saving the *Shenandoah,* none of them were able to state a definite increment of speed which they considered would have been beneficial to the *Shenandoah* under the actual storm conditions of the disaster. They further did not know whether such speed increment actually remained unused in the *Shenandoah's* power plant capabilities. It is the opinion of the survivors that the presence and operation of a sixth engine in the *Shenandoah* would not have aided her in any way to escape, once caught in the squall. The argument of greater power in the *Shenandoah,* unsupported by the other actual conditions under which the *Shenandoah* was lost, can be offset by the simple statement, absurd as it may sound, that had the *Shenandoah* had *less* power, she would not have reached the scene of the disaster-bearing storm at the time she did, and, therefore, it might have been said just as well that *lower* power might have been of benefit to the *Shenandoah's* welfare. It is a well known fact that in airship operation a reduction of airspeed when laboring through turbulent air reduces the aerodynamical stresses on the hull. While in general, the question of high power in airships is open to debate, it is certain in our minds that the absence of the sixth engine in the *Shenandoah* was not a contributing factor in her disaster. It may be pointed out that the *R-38,* which was known as the *ZR-2,* was undoubtedly broken and lost because of the exceptionally high

power employed with her structural strength. It is also a matter of record in the findings of the investigation into the loss of the *Roma* that the substitution of engines of greater power than those originally intended for installation was a contributing factor to the loss of the *Roma*.

PARACHUTES.

The subject of the use of parachutes on rigid airships, particularly on the *Shenandoah*, has arisen before this Court. Although the *Shenandoah* actually carried some parachutes, they were found after the wreck in their habitual place of stowage. Although while in such stowage they were readily accessible to anyone who might have desired to use them, and though practically every person in the *Shenandoah* was aware of the seriousness of the situation, there has not been mentioned in a single instance any desire, intention or effort to use a parachute for any purpose whatsoever in this disaster. As a matter of fact, it would have been impossible for any member of the *Shenandoah* crew to perform his duties constantly wearing any type of parachute yet devised. Furthermore, had sufficient parachutes for the entire crew been provided in definite locations, the suddenness of the disaster would have made it impossible to utilize such parachutes. Had even those officers and men in the control car been equipped with parachutes, it is our opinion that they would have been unable to escape therefrom to use their parachutes. Furthermore, it has been pointed out that in case of an impending airship disaster, there might be those of the crew who would abandon ship or resort to the use of parachutes at a premature time, or at a time when their presence on the ship at their stations might provide the necessary margin of safety.

MISCELLANEOUS.

Captain Heinen made other bold statements that require refutation. In reference to the *Shenandoah* disaster he admitted having said "but every man at the Air Stations knows why it happened." After questioning it developed that his expression "every man" was a gross exaggeration that really included only two or three men. Continuing, he testified "that has proved to me conclu-

sively that the condition of the ship was very well known and very much feared by the crew, as I found out later after the accident."

Upon being forced by the Court to give the names of those involved by his statement above, he mentioned a total of three: Aviation Machinist's Mate 1st class Hereth, James Work, a civilian employee, and Boatswain Buckley.

Further examination of Hereth revealed that after his return to Lakehurst from the scene of the disaster, he, one day, somewhat under the influence of liquor, accidentally encountered Captain Heinen, who questioned him about the disaster. It developed that Hereth told him precisely what he has stated before this Court, and reference to Hereth's testimony shows that he certainly had no knowledge of how the disaster occurred. It is almost incredible that a man so proud to say that had he been in command of the *Shenandoah* the accident could not have happened should have to rely upon the statement of an intoxicated person to substantiate his own statements.

James Work, appearing as a witness, testified that he did have conversations with Captain Heinen, but denied having told him "that every man at the station knows why it happened," and told how he had merely explained the valve changes on the day of the disaster to a number of persons, including Heinen.

When pinned down to name anyone who feared to fly in the *Shenandoah,* Captain Heinen named one man, Boatswain Buckley. The absurdity of this hearsay statement was exposed when Lieutenant Bauch later testified that Boatswain Buckley, although not a member of the *Shenandoah* crew, had even requested to be allowed to make the Mid-West flight in any capacity whatsoever.

By logical deduction from the evidence before this Court, it is clearly established that the *Shenandoah* was destroyed by being broken in two and precipitated to the ground by the aerodynamical stresses imposed upon her by the vertical currents of the squall in which she had been entrapped without warning. Upon finding the ship in this predicament, the Commanding Officer did everything known to airship science to save his ship. It is unnecessary to more than point out the pioneer status of airship operation. Cumulative evidence, as pointed out heretofore, has been adduced to

show that there were no positive danger signs to warn the Commanding Officer of impending danger. The analogy is clearly that of a surface vessel striking an iceberg without warning. It is granted that by Navy Regulations the Commanding Officer is always responsible for the safe conduct of his ship, but it is contended that, in such a case as that of the loss of the *Shenandoah*, the responsibility of the Commanding Officer terminates at some particular point. Such a point must have been that when everything known to mankind had been resorted to in the effort to save the ship. Indisputable evidence shows that such was the case, and that in spite of heroic measures taken the disaster could not have been averted. The category, then, into which the *Shenandoah* must fall is that of an "Inevitable Accident." For such a case we have the precedent cited in the "Naval Digest, 1916," page 87, paragraph 17, beginning with the 22nd line: "To admit of that defence (inevitable accident) it must appear that the danger was not to be apprehended, or, if it was liable to arise, that a proper watch was kept beforehand and *seasonable* precautions taken against such a liability and that reasonable skill was used when danger arose. (The Columbia, 48 Fed. Rep., 325.)"

We submit that every requirement of the above decision was fulfilled in the case of the loss of the *Shenandoah*, for having exhausted every possibility and having used the utmost skill toward saving the vessel in danger that was not to be apprehended, the responsibility of the Commanding Officer ended.

Nor can there be any added responsibility for the loss of *life* incidental to this disaster, for such is inseparably connected to the loss of the vessel. The only possible point that can be raised in this connection is the question of whether parachutes would have been of any value under the circumstances in saving lives. We believe it sufficient merely to call attention here to the preponderant and positive testimony of a number of witnesses well qualified to know, as opposed to the fancies of other witnesses whose testimony was not based on a knowledge of operating conditions, that parachutes would not only have failed in this case to save lives, but might easily have imperiled the lives of others who were actually saved by staying with the ship.

Although we are survivors of one of the worst air accidents that has ever occurred, we nevertheless have not lost faith in rigid airships and are still of the opinion that rigid airships are of a great deal of value for naval and commercial purposes as well. It would be in keeping with neither tradition nor sound business to abandon at this stage an enterprise on which such an immense amount of effort has already been expended and which with slightly more expenditure along the present lines, will bring forth the potential value and possibilities of this type of aircraft. We therefore take this opportunity of stating that we are ready to do and give our utmost toward the continuation and furthering of this project and we urge upon this Court the advisability of its recommending the retention, continuation and furtherance of rigid airship development and operation in the Navy.

SUMMARY BY COMMANDER S. M. KRAUS, U. S. NAVY, REPRESENTATIVE OF THE CHIEF OF THE BUREAU OF AERONAUTICS.

There are two points which the summaries of Professor Hovgaard and of the survivors have suggested to me as possibly needing, not further explanation, but perhaps a slightly different explanation.

I have in mind the testimony of Aviation Chief Rigger Collier, in regard to cells 8 and 9 of the *Shenandoah*. I would point out to the Court that this witness, by his own testimony, was asleep until almost the instant of the breaking up of the ship. The question then arises whether he then actually saw conditions concerning which he has testified.

To my mind, that cell 8 should have been full after having descended some 3000 feet from the earlier altitude is, in effect, almost a physical impossibility.

Should the Court accept as a fact a condition or situation almost a physical impossibility, I feel that I should point out that the conclusion of the technical adviser to the Judge Advocate, in his argument, showed that manual operation of valves constitutes a system which does not involve danger, and that the charge that it involves danger is not borne out.

We have here in the case of cells 8 and 9 two cells of almost equal size, side by side in the ship, exposed under the same conditions, and both equipped with automatic valves, and yet of all the cells in the ship, and of all the valves, there has been no serious question as to the uniformity of action of any valve on the ship to any of the cells except in 8 and 9, both equipped with automatic valves.

One of the subjects that perhaps calls for some few remarks on my part, again suggested to me by the summary of the technical adviser to the Judge Advocate, is that he has found no explanation for the reported snapping of wires at or near the time the ship was at the top of the rise. I believe it well to point out that there has never been presented before this Court any other plot or diagram of the ship's position in altitude during this position except a space time diagram. I would suggest to the Court, in considering this particular subject with a view of obtaining light on what appears now to be an obscure matter, and since it still has access to the services of the technical adviser of the Judge Advocate, that the data on which this existing curve has been built up be replotted to co-ordinates of velocity versus time; and then further plotted to co-ordinates of acceleration versus time. I believe such a plot of this data would be very illuminating, and that the technical adviser to the Judge Advocate, as well as the Court itself, will then observe that accelerations and change of sign of acceleration in the whole mass of the ship existed at that moment, and of such a high order as to presuppose a shifting and rendering of all the wiring in the ship, and it may well be even of such magnitude as to involve the parting of certain wires; that the ship at that time did not break was due solely to the fact that these accelerations, although nearly as great as those that later caused the destruction of the ship, were diminished in their effects by the action of gravity on most of those masses in the ship. On the final rise of the ship immediately preceding her destruction, unfortunately, the forces engendered by the accelerations then impressed upon the mass of the ship were additive to the forces of gravity as well as to the dynamic forces involved, and the ship failed. I believe it is desirable that the two points on which I have touched be carefully examined in the light of these few remarks.

[An attempt was made later to replot the record of the aerograph on the above co-ordinates, but the small scale of the record precluded such a re-plot. See calculations of C. P. Burgess.]

FINDINGS OF THE COURT.

The Court having thoroughly inquired into all the facts and circumstances connected with the loss of the U.S.S. *Shenandoah* and having considered the evidence adduced, finds as follows:

I. MATTERS ENTIRELY ANTECEDENT TO THE INCEPTION OF LAST TRIP OF "SHENANDOAH," BUT HAVING A MATERIAL BEARING ON THIS INQUIRY.

FACTS.

F–1. The design of the *Shenandoah* was begun under the cognizance of the Bureau of Construction and Repair in 1919. When the Bureau of Aeronautics was created and organized in 1921, all matters connected with the design and construction of the airship were transferred to that bureau.

F–2. At the request of the Bureau of Aeronautics, the National Advisory Committee on Aeronautics appointed a technical committee of five, composed of scientists and engineers of national reputation, to examine the design methods and assumptions used as well as the actual design of the *Shenandoah*. This committee after prolonged study completely approved the design of the airship as prepared.

F–3. The airship was built at the United States Naval Air Station, Lakehurst, N. J., was completed in 1923, made its first flight on September 4, 1923, and was commissioned as the U.S.S. *Shenandoah* in October of the same year.

F–4. In the design of the *Shenandoah* approximately 4 per cent more of the total lift was assigned to the hull structure and fittings than in the design of certain foreign rigid airships for which comparative figures were available. As finally completed, the weight of the ship exceeded the designed weight by approximately 1 per cent.

F–5. The only major casualty suffered by the *Shenandoah* prior to the start of the Mid-West flight occurred in January, 1924,

when it was torn from the mooring mast at Lakehurst by stress of weather. The ship was thoroughly inspected and repaired after this accident.

F–6. Prior to the Mid-West flight, the *Shenandoah* had cruised a total of approximately 740 hours in the air, covering approximately 25,835 statute ground-miles, of which 605½ hours and 20,761 miles, including a transcontinental flight, was made subsequent to January 16, 1924.

F–7. A majority of the officers and crew had served in the *Shenandoah* continuously from the time of commissioning up to and including the Mid-West flight of September 2, 1925. Many of these had received previous training on rigid airships abroad.

F–8. In accordance with the provisions of the National Defense Act of 1920 and upon recommendation of the Joint Army and Navy Board approved by the Secretaries of War and Navy, the Navy was charged with the sole responsibility for the procurement and development of rigid airships for use over both land and water.

F–9. The U.S.S. *Los Angeles* was obtained through an international agreement that it should be used for commercial purposes only and was assigned to the Navy for operation. The Navy is charged with the development of rigid airships for commercial as well as for military purposes.

OPINIONS.

O–1. The *Shenandoah* represented the best practices in design and construction at the period of its building.

O–2. The proportionately greater weight for structure and fittings of the *Shenandoah* resulted in increased strength.

O–3. The damage to the *Shenandoah* due to breaking away from the Lakehurst mooring mast was fully repaired and no loss of strength ensued as a result thereof.

O–4. The officers and crew of the *Shenandoah* were skilled and competent operators of rigid airships.

O–5. The operations of rigid airships over land as well as over the sea is a proper and lawful function of the Navy.

II. MATTERS LEADING UP TO THE FLIGHT OF THE "SHENANDOAH" BEGINNING SEPTEMBER 2, 1925.

FACTS.

F–10. The Navy Department had received 248 requests from public officials, public and semi-public bodies, and individuals for a flight of a rigid airship over the Middle West.

F-11. On June 7, 1925, in obedience to orders from the Navy Department, the *Los Angeles* started a flight to Minneapolis, but, due to engine trouble, returned to Lakehurst in accordance with the decision of the commanding officer after completing only the first part of the flight.

F–12. On June 9, the Chief of Naval Operations informed the commanding officer of the United States Naval Air Station, Lakehurst, and the commanding officer of the *Shenandoah* that that airship, which was then deflated, would be ordered to make the trip previously scheduled for the *Los Angeles* as soon as the former could be made ready, helium transferred, and the airship placed in active service.

F–13. In letter dated June 15, the commanding officer of the *Shenandoah* recommended that such a flight be postponed until September on account of the probability of unfavorable weather conditions in the Mid-West during July and August. Prior to the receipt of this letter the Chief of Naval Operations had issued orders dated June 19 for a flight over the Mid-West by the *Shenandoah* as soon as ready.

F–14. Upon its receipt, the letter of the commanding officer of the *Shenandoah* of June 15 was considered by both the Chief of Naval Operations and the Chief of the Bureau of Aeronautics. As a result, the commanding officer of the *Shenandoah* was ordered to Washington for consultation. On June 30 the consultation was held, at which the Chief of Naval Operations, the Chief of the Bureau of Aeronautics, the commanding officer of the *Shenandoah,* and the head of the ships' movement division of operations were present.

F–15. As a result of this conference the orders of June 19 were revoked.

F–16. A mooring mast for rigid airships had been erected at Detroit, Mich., by Mr. Henry Ford, and was completed about July 9. The Navy Department had been requested to send a rigid airship to Detroit to test this mast, the first to be erected in this country by private enterprise.

F-17. In letter dated July 20, the Chief of Naval Operations directed the commanding officer of the *Shenandoah* to submit an itinerary for a Mid-West flight to pass over as many of certain specified cities as practicable on dates in the early part of September, when State and county fairs were scheduled to be held and to test the mooring mast at Detroit.

F–18. In reply dated August 4 the commanding officer of the *Shenandoah* recommended two separate flights — the first during the last week in August to Detroit to test the new mooring mast, and the second beginning September 7 over the entire route covering the specified cities.

F–19. The Chief of Naval Operations, after consultation with the Chief of the Bureau of Aeronautics, directed one flight only in lieu of two, this flight to begin September 2. The orders dated August 12 directed this flight.

OPINIONS.

O–6. The commanding officer of the *Shenandoah* was fully consulted and given every opportunity to state his views in regard to the Mid-West flight.

O–7. The commanding officer of the *Shenandoah* did not at any time express opposition to, or any reluctance in, undertaking the flight beginning September 2 on the ground of possible adverse weather conditions.

O–8. The object of the Mid-West flight was threefold, viz., to test the mooring mast at Detroit, to continue training of personnel, and to comply with the many requests received from citizens of that section of the country that such a flight be made.

O–9. While recognizing the propriety and necessity of the legally constituted authorities in the naval service being the sole judges of the sufficiency of the reasons actuating all orders to naval craft and further recognizing that the practice of ordering movements of naval vessels for the purpose of complying with

public requests is in accord with long-established customs, it is considered that such movements should be limited to essentially naval and military operations in so far as possible, especially in the case of new and experimental types.

III. THE CONDITION OF SHIP AND CREW AT TIME OF FLIGHT.

FACTS.

F–20. The ship was fully manned and equipped and in material readiness for flight.

F–21. Frequent and careful inspections of the entire structure of the ship had been made throughout its life. In addition, material similar to that in the structure of the ship had been kept under observation and been subjected to tests under instructions of the Chief of the Bureau of Aeronautics.

F–22. Extensive tests of material selected from widely separated parts of the structure after the wreck were made by the Bureau of Standards.

F–23. Both sets of tests have shown a limited deterioration of the material in the form of intercrystalline corrosion, which deterioration to the extent found does not affect in any substantial degree the ultimate tensile or compressive strength or the modulus of elasticity of the material, but does largely reduce its ductility.

F–24. The design of the structure of the *Shenandoah* was based on ultimate failure of individual girders in compression. The design of these individual girders was such that under compression they would fail through flexure by instability. This failure would occur before the stress in the separate members of the girder had reached the elastic limit of the material and was therefore dependent upon the modulus of elasticity only and not upon the ultimate strength of the material.

F–25. In letter dated September 16, 1924, the commanding officer of the *Shenandoah* first proposed certain changes in the system of gas valves of that ship and subsequently renewed his recommendations in letters dated December 15, 1924, January 9, and May 12, 1925, and in telephone conversation of May 28, 1925. The commanding officer of the Naval Air Station, Lakehurst, concurred in this recommendation. Although the Bureau of Aero-

nautics at first questioned the advisability of this change, it finally approved it in letter of May 28 as an experimental installation, and in letter of June 13 transmitted to the commanding officer of the *Shenandoah* a design memorandum showing the effect of this change in reduction of the maximum safe rate of rise above pressure height of an airship so equipped. This change, involving a reduction in number of automatic valves, was made the latter part of May and early part of June. As a result the *Shenandoah* was equipped with automatic valves in gas cells Nos. 4, 5, 8, 9, 12, 13, 16, and 17 and with maneuvering valves in gas cells Nos. 4, 6, 7, 10, 11, 14, 15, and 18. Subsequent to this change and prior to the Mid-West flight the *Shenandoah* had cruised about 186 hours, covering about 6750 statute ground miles, not including Mid-West flight.

F–26. Certain other changes not contemplated in the original design, including removal of No. 6 engine, installation of water-recovery apparatus, removal of manometers due to the fact that no entirely satisfactory type has yet been developed, and other minor alterations were made subsequent to the completion of the *Shenandoah*.

F–27. The *Shenandoah* was equipped with only two parachutes, which were carried for the purpose of landing personnel in order to make ground preparations in case it became necessary on any flight to make an emergency landing at a point where preparations had not previously been made.

F–28. The manual of the Bureau of Aeronautics required—

"*Parachutes.*—(a) Parachutes will be worn by all personnel in all types of aircraft when available. Specific exemptions may be made by commanding officers when their use increases the hazards of the personnel."

The established practice at the Naval Air Station, Lakehurst, on both the *Shenandoah* and the *Los Angeles* was to carry only two parachutes.

F–29. The officers and crew of the *Shenandoah* were well organized, well trained, and highly skilled in the performance of their duties.

OPINIONS.

O–10. Deterioration of structural material was not a direct or indirect cause of the loss of the ship.

O–11. The deterioration shown to exist was not due to local causes, but was generally distributed throughout the structure. It was not due to any lack of care or inspection in its manufacture and fabrication, nor to neglect of proper measures for care and preservation in the ship after completion.

O–12. The Court concurs in the opinions of expert witnesses that the rate of deterioration would not affect the safety of the structure of an airship for a much longer period than that covered by the life of the *Shenandoah*.

O–13. Material of this character will, until more thoroughly known and understood, require careful handling and observation during fabrication and while in service, but no evidence adduced before this Court indicates that its use in the structure of rigid airships is unsafe or inadvisable.

O–14. The Court concurs in the opinions of all qualified experts in the operation of rigid airships, who appeared before it, that the wearing of the present type of parachutes would add to the general risks and hazards of handling such ships due to their interference, especially in emergencies, with the quick and efficient performance of duty by the personnel. The Court is therefore of the opinion that the judgment of the commanding officer in regard to carrying parachutes was sound. The present practice is adequately covered by the paragraph in the manual of the Bureau of Aeronautics quoted in F–28.

O–15. None of the changes referred to in F–26 were contributory to the loss of the ship. The successful development of a thoroughly dependable type of manometer or gas-pressure gauge giving accurate readings in the control car for each gas cell would be a contribution to safe operation of rigid airships.

O–16. Every effort should be made to develop and perfect such type of parachute as could be worn in an emergency without undue handicap in the performance of duty.

(NOTE.—See O–23 and O–27 for opinion on change in valves.)

IV. THE FLIGHT FROM START TO ABOUT 5:22 A.M. (EASTERN STANDARD TIME), SEPTEMBER 3, 1925.

FACTS.

F–30. The flight was made in obedience to lawful orders, issued by competent authority and containing a prudential clause as follows:

"Should the dictates of safety and the weather conditions existing make it advisable, the commanding officer of the *Shenandoah* is authorized to make such modifications in the above itinerary as he deems necessary, remembering, however, that this route will be published in the press and that many will be disappointed should the *Shenandoah* fail to follow the approved schedule."

F–31. Before leaving the Naval Air Station, Lakehurst, the commanding officer of the *Shenandoah* obtained and considered in consultation with the aerological officer of the ship the latest weather maps and reports.

F–32. The *Shenandoah* left the mooring mast at the Naval Air Station, Lakehurst, at 2:52 p.m., September 2, and proceeded on the Mid-West flight.

F–33. The 8 p.m. weather reports were received, mapped, and studied by the aerological officer about 11:30 p.m., September 2. The scheduled weather report from Lakehurst for the region being traversed was received about midnight. These reports were discussed by the aerological officer with the executive officer who was on duty in the control car.

F–34. Between 3 and 3:30 a.m. the commanding officer, executive officer, and aerological officer, all of whom were in the control car, observed and discussed the first apparently unfavorable weather conditions. For the next two hours the weather conditions were under constant observation and discussion by the commanding officer and the aerological officer, who were in complete agreement in their conclusions except at one point, when the aerological officer advised a change of course to the south. The commanding officer, after consideration and discussion, which developed that the aerological officer based his opinion not on any

close or immediate danger, but on the unusual movement of storm areas 25 to 50 miles to the north, decided to adhere to the course of 260 degrees true.

OPINIONS.

O–17. The commanding officer was entirely justified in starting the flight at the time chosen, as the weather maps and reports indicated nothing which would have rendered it unsafe or inadvisable.

O–18. The weather reports which were received and mapped between 11 p.m. and midnight, September 2, indicated nothing which rendered it unsafe or inadvisable to proceed.

O–19. A more extensive meteorological service, which would require more weather stations and more frequent broadcasting of reports, probably would greatly have contributed to the safe navigation of the *Shenandoah* on this trip.

O–20. Although subsequent events showed that a change of course to south when suggested by the aerological officer would have been advisable, the commanding officer's decision to maintain his course was a matter for his decision only, was made on his best judgment after discussion and consideration, and was based on the facts and conditions as then known or observed. Any error of judgment involved in this decision was entirely without negligence or blame.

V. THE FLIGHT FROM 5:22 A.M. TO 5:47 A.M. (EASTERN STANDARD TIME).

FACTS.

F–35. During this period constant watch was kept on aerological conditions by the commanding officer and the aerological officer, but none of the usual cloud formations indicating abnormal air currents in the immediate vicinity of the ship were observed.

F–36. Prior to arrival at pressure height all valve covers had been removed.

F–37. At the beginning of this period the *Shenandoah* was proceeding on a course 300 degrees true at a height of 1800 feet, but was making little or no ground speed, due to strong adverse winds.

The airship at this time had a pressure height of 3800 feet. At 5:22 a.m. the airship began to rise under the influence of vertical air currents; rose to a height of 3150 feet in eight minutes; was brought under control and steadied at that level for six minutes; then rose again and more rapidly to 6100 feet in 10 minutes; dropped rapidly to 3000 feet in three minutes; and finally rose again, sharply up by the nose, to probably 3700 feet, at which point the ship broke. This last rise was accompanied by a movement of rotation of the whole ship in a horizontal plane and by violent rolling and pitching. During all these movements the ship was out of control.

(NOTE.—All the above figures are approximate only.)

F–38. The average rate of rise during the first part was 170 feet per minute; during the second part 300 feet per minute, with a probable maximum of 700 feet in the last 42 seconds. The average rate of fall was 1000 feet per minute. The rate of the final rise is not ascertainable, but is immaterial, as the ship broke before reaching pressure height.

F–39. The existence of high velocity vertical air currents without accompanying cloud formations or other visible indications is unusual, but is a phenomenon known to meteorologists.

F–40. During this period of uncontrolled movements various recognized methods for bringing the ship under control, such as opening the maneuvering valves for five minutes, nosing down by means of controls, speeding up engines and letting go ballast were utilized, but without result so far as obtaining full control of the movements of the ship was concerned.

F–41. The following sequence of events in the final destruction of the ship is indicated by the evidence.

At or about the time the ship was at its highest altitude unusual sounds within the body of the ship were heard. These sounds are variously described by different witnesses, but seem clearly to indicate the existence at that time of unusual stresses in some part or parts of the structure. The sharp rise by the nose, the rotation in horizontal plane, and the rolling and pitching of the ship during final uncontrolled rise indicate the existence of large unbalanced external aerodynamic forces acting on the ship at that time. The next abnormal event was a cracking of the struts of the control car.

(See F–45.) The first break in the main structure of the ship occurred between frames 120 and 130, resulting in separating the ship in two parts, the control car remaining attached to the forward section. In something less than a minute thereafter the control car broke loose from the forward section and dropped to the earth. About the same time a second break occurred in the after section of the ship between frames 100 and 110. The midships section thus broken off, with power cars Nos. 4 and 5 attached thereto, dropped almost directly. The after section dropped more slowly and was carried by the wind a distance of about one-third mile before reaching the earth. The forward section was operated as a free balloon by the personnel left therein and was landed safely about 12 miles from the scene of the disaster.

OPINIONS.

O–21. During this period the measures taken by the commanding officer to bring the *Shenandoah* under control were in accordance with the best established practice.

O–22. During this period of extreme danger the conduct of all officers and men was deserving of the highest praise. The commanding officer showed skill, courage, and fine qualities of leadership. All other officers and men showed an intimate knowledge of their duties, fine discipline and morale, and great courage. The handling of the forward section of the ship as a free balloon by Lieutenant Commander Rosendahl and Lieutenant Mayer was particularly deserving of commendation.

O–23. Although testimony of both operating and scientific experts is to the effect that, subject to certain qualifications, gas pressure sufficient to produce permanent strains or actual damage in the structure probably did not exist at the highest point of rise of the ship, the facts (*a*) that sounds of a nature indicating unusual stresses in the structure were heard at this time, (*b*) that the calculations on which expert testimony is based are arrived at through assumptions which are of necessity open to error, and (*c*) that some of these assumptions for their validity depend upon precision in the action of personnel and material, all taken together justify the opinion that gas pressure due to altitude and rate of rise may well have reached values sufficiently high to produce some damage to structure.

O–24. The exact nature or extent of this damage is not ascertainable, but was probably not sufficient to have endangered the ship under normal operating conditions.

O–25. Valve covers having been removed prior to arrival at pressure height, they could not have had any influence on the loss of the ship. Nevertheless, the practice of leaving any valve covers in place at any time after an airship takes the air is considered unsafe and inadvisable.

O–26. The final destruction of the ship was due primarily to large, unbalanced, external, aerodynamic forces arising from high-velocity air currents. Whether the ship, if entirely intact and undamaged, would have broken under the forces existing, or whether prior minor damage due to gas pressure was a determining factor in the final break-up are matters which this Court is unable definitely to determine.

O–27. The change resulting in a reduction of the number of gas valves was inadvisable. The initiation and urging of this change by the commanding officer of the *Shenandoah*, the recommendation for its approval by the commanding officer of the Naval Air Station, Lakehurst, and its final approval as an experimental installation by direction of the Chief of Bureau of Aeronautics appear, in the light of subsequent events, to have been errors of judgment, but were arrived at after full and careful consideration by the most expert officers of the Navy in the operation and design of rigid airships and do not in any way involve negligence or culpability.

O–28. Although the disaster to the *Shenandoah* indicates that a rigid airship can probably be destroyed through external aerodynamic forces only, and that such forces can arise from air currents unaccompanied by visible signs recognizable by aerologists in the present state of knowledge, it is the opinion of the Court that such conditions and combinations of circumstances are unusual and such a conclusion does not, therefore, throw doubt upon the safety and utility of rigid airships to a materially greater degree than does the fact that other types of craft for navigation in the air or on the water are likewise subject to destruction through unusual dangers which cannot invariably be foreseen and provided against.

VI. LOSS OF LIFE.

FACTS.

F–42. The following-named officers and men lost their lives through the destruction of the *Shenandoah:*

Lieut. Commander Zachary Lansdowne, United States Navy.
Lieut. Commander Lewis Hancock, Jr., United States Navy.
Lieut. John B. Lawrence, United States Navy.
Lieut. Arthur B. Houghton, United States Navy.
Lieut. (junior grade) Edgar W. Sheppard, United States Navy.
Allen, Everett P. A.C.R.
Broom, Charles H. A.C.M.M.
Cullinan, James W. A.P.
Joffray, Ralph T. A.R. 1 c.
Mazzuco, Celestino P. A.M.M. 1 c.
Moore, James A., Jr. A.M.M. 1 c.
O'Sullivan, Bartholomew B. A.M.M. 1 c.
Schnitzer, George C. C.R.M.
Spratley, William H. A.M.M. 1 c.

F–43. Chief Gunner Raymond Cole, United States Navy, received slight injuries, and Aviation Chief Rigger John F. McCarthy, United States Navy, was seriously injured.

F–44. Of those lost, eight were in the control car, four in power cars Nos. 4 and 5, and two in the body of the ship at the time of the disaster.

F–45. The control car broke away from the forward section of the ship after the main break in structure, and this minor break was probably a direct consequence of the major break. The method of attachment of the control car to the main structure was the same in its general characteristics as that employed on practically all previous rigid airships, *i.e.*, suspension cables for completely supporting its weight, combined with struts, only sufficiently strong for steadying the car in flight and designed to collapse before injurious stresses could be transmitted to the main structure in case of contact with the ground when making landings. This general method of support is similar to that employed for the suspension of power cars. Two rigid airships designed and constructed since the *Shenandoah* embody a new and improved type

of control car which is directly attached by structural members to the main frame work of the ship.

OPINIONS.

O–29. The deaths of all personnel resulting from this accident were due to injuries received in the line of duty and were not the result of their own misconduct.

O–30. The injuries of all personnel were received in the line of duty and were not the result of their own misconduct.

O–31. Although the design of the control car and its method of suspension were in accordance with sound and well-established practice at the time of the building of the *Shenandoah*, the newer design as embodied in the *Los Angeles* is considered better and safer for future construction.

O–32. Although it is possible that some lives might have been saved had parachutes been carried and been actually worn by officers and crew at the time of the break-up of the ship, this does not affect or modify the opinion expressed in O–14 above, that the use of the present type of parachute in rigid airships is not advisable. The lesser danger must be accepted to avoid the greater risk.

VII. LOSS OF, AND DAMAGE TO, PROPERTY.

FACTS.

F–46. The loss of the *Shenandoah* was complete. Steps for the recovery of salvage value from the wreck have already been taken.

F–47. Minor damage was done to private property belonging to three residents in the vicinity of the disaster.

F–48. A board was appointed by the Secretary of the Navy immediately after the loss of the ship, which was charged with the duty, among others, of making report in regard to damage to private property.

OPINIONS.

O–33. The report of the senior member of that board adequately covers the matter of damage to private property and the recom-

mendations contained therein are sufficient to permit final decision and further action on the claims arising from this source.

VIII. GENERAL.

OPINION.

O–34. It may be accepted in the case of the loss of any craft at a certain place at a certain time that any change of action on the part of any individual directly or indirectly connected with the movement of such craft might have avoided such loss. In this sense, and in this sense only, can allegations of direct or indirect individual responsibility for the loss of the *Shenandoah* and the loss of life consequent thereon be supported. Such actions of individuals must be judged on the basis of the facts and established practices known and recognized at that time rather than on conclusions or deductions drawn after the event. Such actions of individuals as in light of subsequent events have been termed "errors of judgment" were, in the opinion of this Court, clearly without negligence or culpability. The disaster is part of the price which must inevitably be paid in the development of any new and hazardous art.

O–35. The Court is of the opinion that the recommendations of Professor Hovgaard, technical adviser to the Judge Advocate, in his technical analysis of the evidence, and of Lieutenant Commander Rosendahl, senior surviving officer of the *Shenandoah,* in his summing up of the case on behalf of the survivors, both of which recommendations deal with future development of rigid airships, are worthy of the most careful consideration.

O–36. The Court is of the opinion that competent technical and operating officers should be detailed to study the great mass of evidence and exhibits forming a part of the proceedings of this Court in order to collate it in such form as to be most readily available and useful for future reference in any further development in the arts of construction and operation of rigid airships.

RECOMMENDATION.

That no further proceedings be had in the matter.

The above findings and recommendation were approved by the Navy Department.

SOME EXPERIENCES WITH TORSIONAL VIBRATION PROBLEMS IN DIESEL ENGINE INSTALLATIONS.*

By John F. Fox, Mechanical Engineer,
U. S. Navy Yard, New York.

The difficulties which may arise from torsional vibration in internal combustion engine crankshafts have long been recognized. Trouble with automobile and air craft engines has occurred repeatedly on this account, and many mysterious crankshaft failures in Diesel engines could undoubtedly have been traced to this cause had the necessary investigation been made. In view of the amount of literature which has appeared on the subject, it is remarkable that it has received so little attention by qualified oil engine designers. Until recently, the tendency has been to seek the cause of crankshaft failures along metallurgical lines, whereas, as a matter of fact, many failures due to torsional vibration would have occurred sooner or later, no matter how excellent the material.

Owing to their high speed and the arrangement of the rotating masses in the shafting, submarine oil engine installations are almost invariably found to have one or more critical speeds in their operating range. As will be shown later on, any installation consisting of a multicylinder oil engine, closely coupled to a heavy rotating mass, such as a generator, is particularly prone to encounter violent torsional vibrations if required to operate over a broad range of speeds.

A brief description of a few of the problems involving torsional vibration which have been encountered by the New York yard may prove of value. The last, and probably the most interesting, involved the four sea-going hopper dredges recently placed in service by the Engineer Department of the U. S. Army. These dredges are equipped with Diesel electric drive for both propulsion and

* Much of the material in this article was published in "The Military Engineer," November, 1925, and is re-published here through the courtesy of that magazine.

pump operation. The main generating equipment consists of three 1000 B.H.P. full Diesel engines, each driving a 700 K. W. Westinghouse generator. The engines are designed to operate at 150 R.P.M. under control of a governor arranged so as to vary the stroke of the fuel pump. Control of the electrical end of the installation is by the Ward-Leonard system.

The engine rooms of these dredges were particularly large and well laid out, so that all machinery was easily accessible for upkeep and repair. As both engines and generators were of approved construction, there was every reason to expect not only satisfactory, but greatly improved dredge operation.

The Diesel engines were originally designed as direct drive units for the U. S. Shipping Board, but were never utilized as such. They were purchased at reduced cost by the Army for use in the electric drive installation on the dredges. When making the machinery layout there was apparently no attempt made to determine the existence or range of the torsional critical speeds which might be encountered when the engines were close coupled to large revolving masses such as generators instead of to a long and more or less flexible line shaft with a propeller at the end.

The experience of the Navy with the problem has demonstrated that such calculations are fundamental and should be correctly made before any decision on a design is made or money invested in building.

The Diesel dredges on completion apparently passed satisfactory trials and one of them was despatched to Florida for dredging work while the remaining three were retained in the vicinity of New York. A few hundred engine hours after being placed in service the New York Yard's consulting Metallurgist, Dr. William Campbell, was communicated with by the office of the First District Engineer, U. S. A., in regard to a broken crankshaft on the *W. L. Marshall.* From the information received, Dr. Campbell advised them to get in touch with the Navy Yard, New York, as his experience had shown that the material was usually sufficiently good and that the cause of failure was more frequently faulty design.

The yard's representative visited the *Marshall* and found a typical torsional shaft fracture in the journal immediately forward of

No. 6 crank, that is, the crank next the flywheel. The break extended at a 45 degree angle across the journal through the oil hole. So many fractures of this type had been investigated that there could be no question as to its cause, and the Army was requested to remove bearing caps on the after sections of all shafts of all dredges and examine them carefully. Three of the dredges examined had cracked shafts but the fourth was apparently unharmed. Investigation of conditions on the latter dredge developed the fact that the dredge engineer had noted a peculiar sound in his engines at 150 R.P.M. and had not operated them above 140 R.P.M., which happened to be sufficiently clear of the range of the sixth order critical speed to avoid damage.

As stated above one dredge had not broken any shafts so as a check on the preliminary diagnosis torsiographic records were taken on this ship. These records showed that the governor, when set as designed, was holding the engine in the range of a violent 6th order critical speed whose peak was at about 150 R.P.M. Figure 10 shows the graphs taken from the end of the engine farthest away from the generator and gives a rough idea of the vibration of the engine crankshaft at 150 revolutions per minute compared with other speeds at which normal operation was obtained.

For a clear understanding of the problem presented by the dredges, it is necessary to outline briefly the following elementary principles:—

The principal shafting vibrations are transverse and torsional. Torsional vibrations consist of a twisting and untwisting of the shaft about a longitudinal axis. Transverse vibrations consist of a transverse motion at right angles to the longitudinal axis of the shaft. Torsional vibration is not related in any way to the phenomena of shaft whirling.

Consider the shaft and masses in Figure 1. The arrangement in Figure 1 comprises two masses M—1 and M—2 connected by a length of shafting "*l*" supported in two bearings. It is obvious that if we grasp the two masses, twist them in opposite directions and suddenly release them, that they will continue to oscillate in a definite period, and with a simple harmonic motion. While one mass is moving in one direction at any given instant the other is moving in the opposite direction. Let us lay out on vertical ordin-

ates above a base line $a - b$ Figure 1, equal to the length of the shaft, the angular deflection at several points along the length of the shaft at any given moment. Let us call deflection in one direction positive and in the other direction negative. We will obtain the curve shown at the bottom of Figure 1. The point at which the curve crosses the shaft is the point at which the angular deflection changes from positive to negative, or in other words, the point at which there is no deflection. This point is called the node.

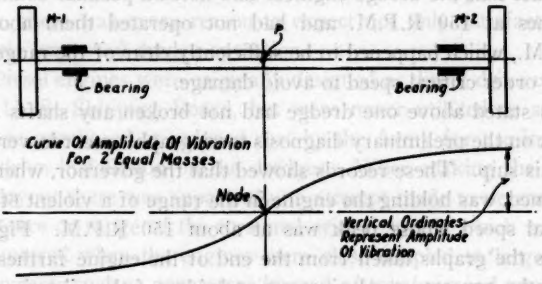


FIGURE 1.

As the shaft vibrates with a simple harmonic motion the movement at any point, with respect to time or displacement can be represented by a sine curve—Figure 2. Along the base line a—b lay off to scale a distance between points (a) and (b) equal to 360 degrees, or $(2\pi$ radians). Between points (a) and (b) lay out a since curve with a maximum ordinate $x - y$ equal, in linear units to the maximum amplitude of the vibration of mass M—1 Figure 1. Call this maximum amplitude θ_0 degrees. Call the number of vibrations per second (N) remembering that one vibration consists of a movement of mass M—1 through an angle of θ_0 degrees in a positive direction to point X back in a negative direction to point Z, θ_0 degrees in the opposite direction and finally to point (b) which is the starting point of the next vibration. The time of one vibration T is equal to $\frac{1}{N}$. Assuming that the mass M—1 has moved through an angle of θ degrees in any given time t from the beginning of the vibration, the relation between θ and θ_0

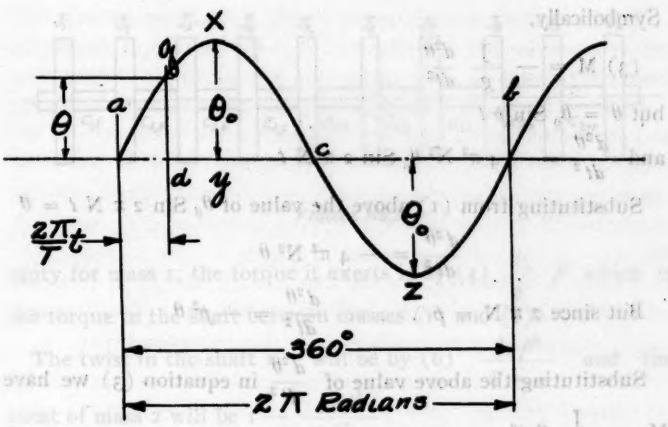


Fig 2

is given by $\theta = \theta_0 \times \text{Sin (distance ad)}$. As the time of one vibration from a to b is equal to T seconds and the time to move from a to d is equal to t seconds, distance ad is equivalent to

$$\frac{t}{T} \times 2\pi \text{ or } \frac{2\pi t}{T}$$ and the relation between θ and θ_0 can be expressed by

(1) $\theta = \theta_0 \text{ Sin } \frac{2\pi t}{T}$ or, since $T = \frac{1}{N}$, $\theta = \theta_0 \text{ Sin } 2\pi N t$

Call $\frac{2\pi}{T} = 2\pi N$ the frequency constant and denote it by (p)

Then formula (1) becomes

(2) $\theta = \theta_0 \text{ Sin } p t$

Turning to Figure 1 let J be the mass polar moment of inertia of mass M_1. If it is vibrating with a maximum amplitude of θ_0 degrees, the torque it exerts on the shaft is equal to the product of the mass polar moment of inertia J and the angular acceleration divided by the gravitational constant (g)

Symbolically,

$$(3)\ M = -\frac{J}{g}\frac{d^2\theta}{dt^2}$$

but $\theta = \theta_0 \operatorname{Sin} p\, t$

and $\frac{d^2\theta}{dt^2} = -4\pi^2 N^2 \theta_0 \operatorname{Sin} 2\pi N t$

Substituting from (1) above the value of $\theta_0 \operatorname{Sin} 2\pi N t = \theta$

$$\frac{d^2\theta}{dt^2} = -4\pi^2 N^2 \theta$$

But since $2\pi N = p$ . . . $\frac{d^2\theta}{dt^2} = -p^2\theta$

Substituting the above value of $\frac{d^2\theta}{dt^2}$ in equation (3) we have

$$M = \frac{J}{g}\theta p^2$$

When $\theta = \theta_0$

$$(4)\ M = \frac{J}{g}\theta_0 p^2$$

In any shaft having a length l, a shearing modulus of elasticity G and a polar moment inertia H. the twist θ, under an applied twisting moment M, is represented by the well-known formula.

$$(5)\ \theta = \frac{M\,l}{G\,H}$$

If for $\frac{G\,H}{l}$ we substitute a constant C we have

$$(6)\ \theta = \frac{M}{C}$$

If the shaft is not of uniform section, we have from (6)

$$(7)\ C = \frac{M}{\theta} = \frac{1}{\sum \frac{e\,l}{G\,H}}$$

the summation being carried out between adjacent masses.

Fig. (3) shows an arrangement of masses on a shaft numbered consecutively. $C_{1,2}$ is the shaft constant for the portion of shaft between masses (1) and (2) and J_1 — J_2 etc., are the respective polar moments of inertia. Assume an amplitude of vibration of

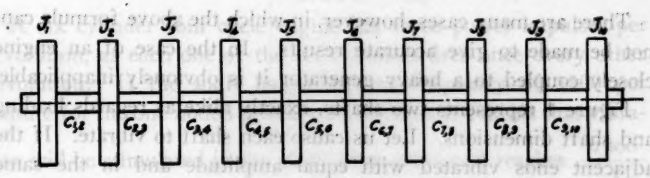


FIGURE No. 2.

unity for mass 1, the torque it exerts is by (4) $\frac{J_1}{g} p^2$ which is the torque in the shaft between masses (1) and (2).

The twist in the shaft 1, 2 will be by (6) $\frac{J_1 p^2}{g C_{1,2}}$ and the twist of mass 2 will be $1 - \frac{J_1 p^2}{g C_{1,2}}$

The torque of mass 2 from (4) will be $\frac{J_2 p^2}{g} \left\{ 1 - \frac{J_1 p^2}{g C_{1,2}} \right\}$

The total torque in shaft 2, 3 will be

$$\frac{J_2 p^2}{g} \left\{ 1 - \frac{J_1 p^2}{g C_{1,2}} \right\} + \frac{J_1 p^2}{g}$$

If the system comprises only two masses, see Figure 1, the above expression can be equated to zero giving

$$p^2 = \frac{g C_{1,2} (J_1 + J_2)}{J_1 J_2}$$

But since . . . $p = 2 \pi N$ by substitution

$$(8) \quad N = \frac{1}{2 \pi} \sqrt{\frac{g C_{1,2} (J_1 + J_2)}{J_1 J_2}}$$

The above is a very well-known formula and is applicable to many practical cases. For example, in the case of an engine and flywheel amidships, coupled to a long length of line shaft with a propeller on the end, we may apply the above formula with a fair degree of accuracy. Consider the moment of inertia of the engine and flywheel equal to J_1 and concentrated at the center of gravity of the rotating and reciprocating masses of the engine. Let J_2 equal the polar moment of inertia of the propeller and $C_{1,2}$ the line shaft constant from propeller to flywheel. Formula (8) can then be applied directly and the result will not be far from the truth.

There are many cases, however, in which the above formula cannot be made to give accurate results. In the case of an engine closely coupled to a heavy generator it is obviously inapplicable.

Figure 4 represents two shafts, exactly alike as regards loading and shaft dimensions. Let us cause each shaft to vibrate. If the adjacent ends vibrated with equal amplitude and in the same phase, we could couple them together and the shaft would continue to vibrate but there would be two nodes instead of only one, as in the case of the shaft represented in Figure 1. By extending the above line of reasoning, we see that a shaft can be in torsional vibration in 1, 2, 3, 4, or, in fact, any number of nodes. Formula (8) is inapplicable to cases where more than one node exists.

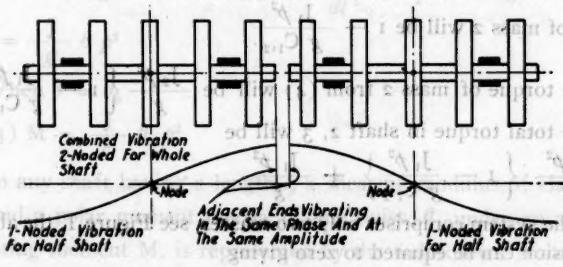


FIGURE 4.

The calculation of the natural frequency of a shaft system in torsional vibration in two or more nodes, or where the engine and generator are closely coupled, is a fairly complex mathematical problem. Not only does the diameter of the shaft vary from one end to the other but the loading varies as well. A reliable method of calculating the frequency for one, two or more nodes, no matter how the shaft is loaded or how its diameter varies from end to end is given by Prof. Frank M. Lewis in the November, 1919, number of the JOURNAL OF THE AMERICAN SOCIETY OF NAVAL ENGINEERS. Recently, Prof. Lewis, has extended his investigations to cover certain other phases of the problem and has given the results of these investigations in a paper read on November 13, 1925, before the Society of Naval Architects and Marine Engineers.

A six cylinder four cycle engine has three power impulses per revolution, as each one of the six cylinders fires once every other revolution. If the shaft has a natural frequency of 1200 per minute for the one noded primary vibration it is evident that vibration will be stimulated in the shaft when the speed reaches $\frac{1200}{3}$ = 400 revolutions per minute. Similarly if the frequency of the secondary vibration for the same engine is found at 2300 per minute vibration will be stimulated in the shaft when the revolutions reach $\frac{2300}{3}$ = 766 per minute. The vibration at 400 revolutions per minute is known as the third order primary vibration and the vibration at 766 revolutions per minute is known as the third order secondary vibration. The third order primary vibration is sometimes met with in the ordinary six cylinder four cycle engine; the third order secondary vibration less frequently.

In our hypothetical engine, vibration will not start at exactly 400 revolutions per minute and 766 revolutions per minute, but at several revolutions below these speeds. Likewise the vibration does not end at these speeds but at several revolutions above them. At 400 and 766 revolutions per minute the number of impulses delivered to the shaft per minute are equal to the natural frequency of the shaft system. Maximum stimulation occurs at these speeds and the vibration attains its greatest amplitude. These speeds are termed critical speeds—see Figure 5. That part of the engine speed range between the start and end of any given order of vibration is known as the range of the vibration. For example in the case of the Dredges under discussion the vibration started at 145 revolutions per minute and ended at 170 revolutions per minute. The range of the vibration was, therefore, twenty five revolutions.

If the six cylinder four cycle engine under consideration is operated, it will be found upon close observation, that there are several points in its speed range where vibration occurs. In the hypothetical 6 cylinder 4 cycle engine, having a natural shaft frequency of 1200 for the one node vibrations, these speeds will be found to be approximately around 266, 200, and 133 R.P.M. While the vibration at these speeds will not be as great as at 400 R.P.M.

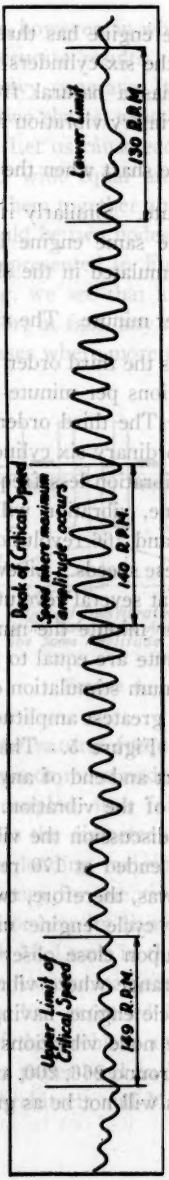


FIGURE 5.

GRAPH FROM TYPICAL DIESEL ENGINE UNIT.

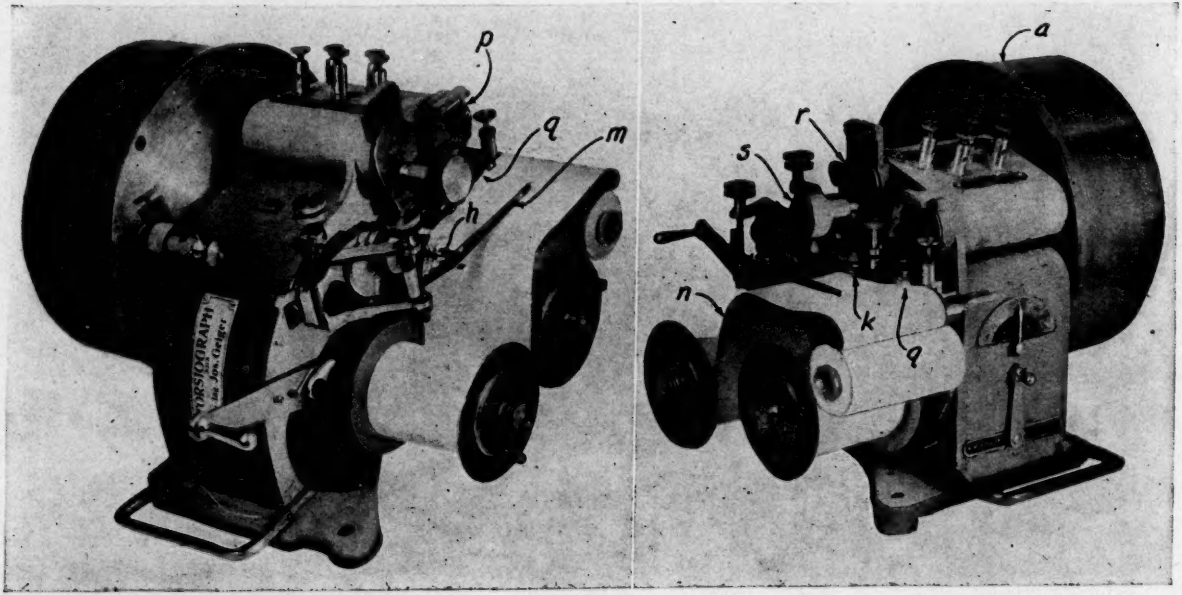


FIGURE 6.

it will, nevertheless, be quite noticeable. These critical speeds can be accounted for by assuming that stimulating forces, giving the crankshaft 4½, 6, and 9 impulses per revolution, come into play. Actually, by means of the torsiograph, to be described later on, it can be proven that the vibration at the above speeds are actually of the 4½, 6th and 9th orders.

The question at once arises as to where forces having a frequency of 4½, 6 and 9 per revolution arise in a six cylinder four cycle engine. The complete analysis of this phase of the problem involves a knowledge of the principle of harmonic analysis. The turning effort exerted on the crankshaft by the acceleration forces of the reciprocating masses alone can be resolved directly into a series of harmonic components. By the application of the well known Fouriers theorem the turning effort exerted by the forces due to gas pressure alone can also be resolved into their harmonic components. If these harmonic components be properly combined it will be found that the harmonic components having the greatest magnitude in a six cylinder four cycle engine, will be of the 3rd, 4½, 6th, and 9th order. There will be found many other harmonic components but the above are those usually encountered. The question of harmonic components, their analysis, and evaluation with respect to the stimulation of the torsional vibration is very thoroughly treated in the Paper read by Prof. Lewis mentioned above so will not be further discussed here.

Before a Diesel engine generating set is placed in operation it is essential that the torsional critical speeds be determined both as regards range and intensity. It is only within the last few years that the apparatus for doing this reliably has been available.

Figure 6 shows the Geiger Torsiograph. This is a German invention intended to record graphically torsional oscillations and operates on a principle similar to that of the seismograph used in detecting earthquakes.

Figure 7 is a diagrammatic sectional view of the torsiograph. (*a*) is a very light aluminum pulley rotating freely on ball bearings (*b*). Concentric with the aluminum pulley and supported on ball bearings (*c*) is a relatively heavy flywheel (*d*). Connecting the heavy flywheel and the light pulley is a long and relatively flexible spiral spring (*e*). It will be evident that if the aluminum pulley

be rotated at a uniform angular velocity, the flywheel will rotate at the same angular velocity as soon as its inertia has been overcome. If the motion of the light aluminum pulley is irregular, that is, if its angular velocity varies, the flywheel will still rotate at practically a uniform velocity, as the irregularities of pulley motion will be absorbed by the flexible spiral spring, and the inertia of the flywheel will cause it to revolve at practically a uniform rate. The flywheel and the aluminum pulley are connected together by links (f) and (g). Link (g) connects to central plunger (h). It will be seen from an inspection of the figure that any motion of the aluminum pulley relative to the heavy flywheel will be translated into a linear movement of the plunger (h).

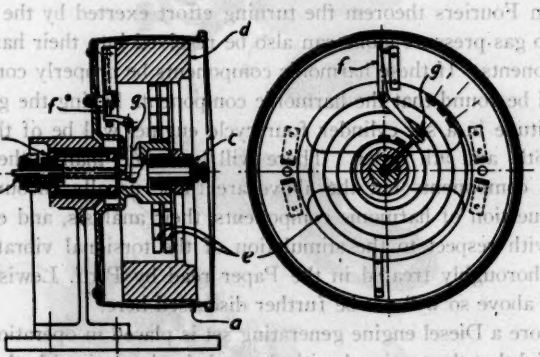


FIGURE 7.

If the aluminum pulley is belted to a shaft rotating non-uniformly, a graphic record of the irregularity of the shaft's motion at the section where the instrument is located, can be obtained. It is necessary, of course, that the belt have no appreciable elasticity or stretch, for otherwise errors would be introduced. A special form of woven canvas belting has been found satisfactory. The motion of the plunger (h) is transmitted to the writing lever (m) Figure 6. The writing lever traces a magnified graphic record of any irregularity in the movement of the pulley on the strip of paper (n) which is moved uniformly by clockwork built

into the instrument. The point of connection of the plunger (h) to the writing lever can be varied so that the magnification of the record may be increased or decreased.

To enable the graphs to be correlated with the dead center positions of the engine cylinders and the engine speed, a time marker, and top center marker are provided. Both of these devices are electrically operated. The top center marker is simply a small electro-magnet carrying an armature (p) to which a pen (q) is attached. The movement of the armature is controlled by an electric contact operated from any convenient point on the engine so as to make and break contact at the top dead center position of one of the engine cylinders. Usually the contact maker can be operated from a spray valve. Figure 8 shows a typical graph; (a) is the

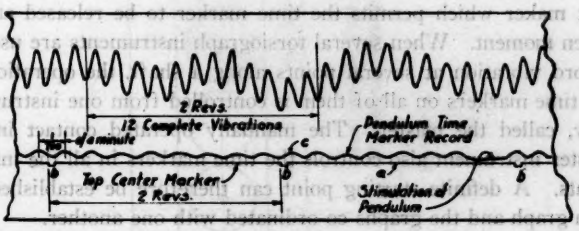


FIGURE 8.

top dead center marker line. The small jogs, b, b, in the line represent the top dead center points marked by the pen when the armature (p) is energized by the contact maker. In this particular case the top-center contact maker was operated from a spray valve. The engine being a four cycle engine, the space between the jogs represents two revolutions. The time marker consists of another electro-magnet and a flat spring (r) on the end of which a weight (a) is attached. The armature also carries a pen (k). If the time marker electro-magnet is energized and then de-energized, it is obvious that the combination of flat spring and weight, making up the armature, will vibrate at a fixed rate, the number of vibrations per second depending on the stiffness of the spring and the size of the weight. The vibrations would, of course, die away after a short time if no provision were made to re-stimulate them. The

torsiograph contains, however, a contact which opens and closes once every revolution of the paper winding drum, periodically stimulating the time marker pendulum. The vibration of the time marker, is, therefore, not allowed to die out. Referring to the typical graph, Figure 8 (*c*) is the line traced by the time marker. The distance between top center points on the graph being known, together with the number of vibrations of the time marker between these points, and the rate of vibration, the speed of the engine in revolutions per minute can be readily calculated for any part of the graph under consideration. In the typical graph it will be noted that there are twelve vibrations recorded by the writing lever between top center marks. As the top center marks are two revolutions apart, the vibration recorded is of the sixth order.

The torsiograph is also provided with a manually operated contact maker which permits the time marker to be released at any given moment. When several torsiograph instruments are used to record vibration at several points along a shaft, the operation of the time markers on all of them is controlled from one instrument only, called the master. The manually operated contact in the master instrument also controls the time markers in all the instruments. A definite starting point can therefore be established on each graph and the graphs co-ordinated with one another.

Figure 9 shows diagrammatically the application of the torsiograph to the investigation conducted on the *Kingman*. Three instruments were used, one at the extreme forward end of the crankshaft belted to a pulley 5.827 inches diameter, one at the after end of the generator shaft, also belted to a pulley 5.827 inches diameter, and one just aft of the flywheel belted directly to the shaft which at this point was fifteen inches in diameter.

As already stated, the graph obtained from the torsiograph is the magnified peripheral displacement of the instrument pulley. Throughout the test on the *Kingman*, a magnification of 3 was used at the forward and aft ends. The dead center markers of the torsiographs were operated from an electric contact located on the spray valve of cylinder No. 1. This arrangement permitted any phase of the shaft vibration to be related to the engine cycle. The vibrations at the three points selected could also be related to one

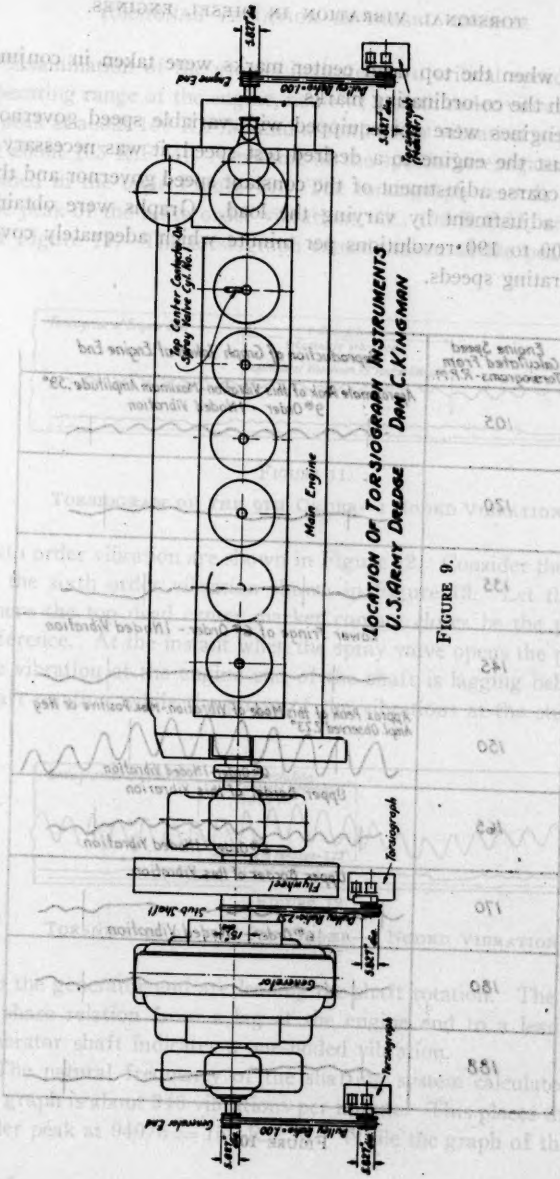

LOCATION OF TORSIOGRAPH INSTRUMENTS
U.S. ARMY DREDGE DAN C. KINGMAN
Engine End
Pulley Data-100
Torsiograph
Main Engine
Flywheel
Stub Shaft
Generator
Engine Speed (Calculated From Torsiograms - R.P.M.)
105
120
135
145
150
165
170
180
188

FIGURE 9.

another when the top dead center marks were taken in conjunction with the co-ordinating marks.

The engines were not equipped with variable speed governors. To adjust the engine to a desired test speed, it was necessary to make a coarse adjustment of the constant speed governor and then a finer adjustment by varying the load. Graphs were obtained from 100 to 190 revolutions per minute which adequately covers the operating speeds.

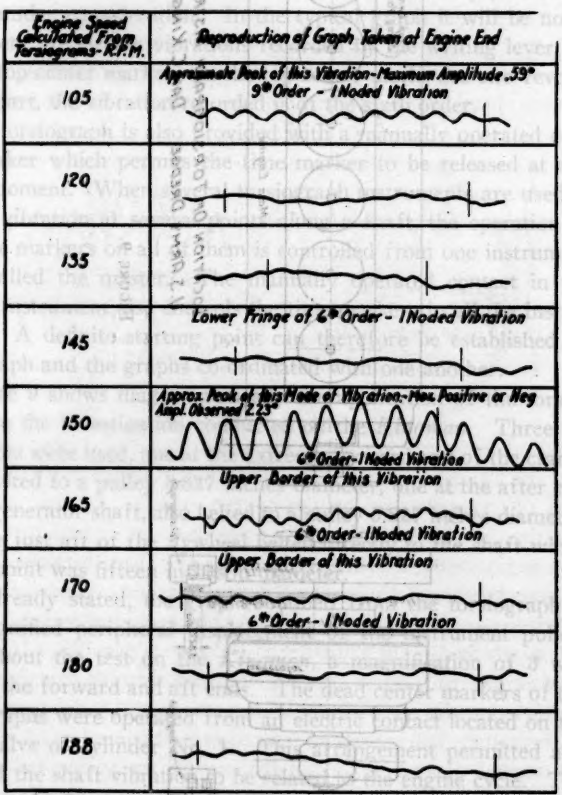


FIGURE 10.

Examination of the graphs indicated two critical speeds in the operating range of the engine, namely; a sixth order vibration with a peak at about 150 R.P.M. and a ninth order vibration with a peak at about 105 R.P.M. A resumé of the speeds investigated is contained in the table Figure 10. The torsiograph records taken at the peak of the ninth order vibrations, *i.e.*, 105 R.P.M. are shown on Figure 11. The torsiograph records taken at the peak of the

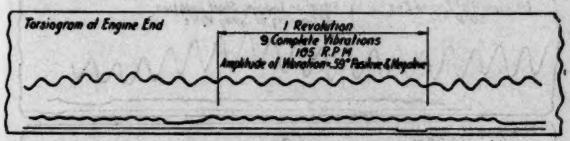


FIGURE 11.

TORSIOGRAM OF THE 9TH ORDER—1 NODED VIBRATION.

sixth order vibration are shown in Figure 12. Consider the graphs of the sixth order vibration shown in Figure 13. Let the point where the top dead center marker contact closes be the point of reference. At the instant when the spray valve opens the phase of the vibration at the engine end of the shaft is lagging behind the shaft rotation, while the phase of the vibrations at the stub shaft

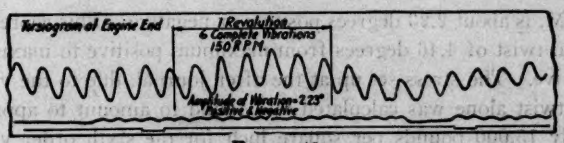


FIGURE 12.

TORSIOGRAM OF THE 6TH ORDER—1 NODED VIBRATION.

and the generator end are leading the shaft rotation. The change in phase relation from a lag at the engine end to a lead at the generator shaft indicates a one-noded vibration.

The natural frequency of the shafting system calculated from the graph is about 940 vibrations per minute. This places the sixth order peak at $940/6 = 156$ R.P.M. While the graph of the ninth

order vibration does not show the phase relation of vibration for the three points along the shaft, it is known to be a one-noded vibration from the frequency which is $9 \times 105 = 945$ vibrations per minute; checking closely with the frequency of the sixth order vibration.

The units had been operated at 150 R.P.M. or almost precisely in the critical speed. The vibratory twist of the shaft at 150

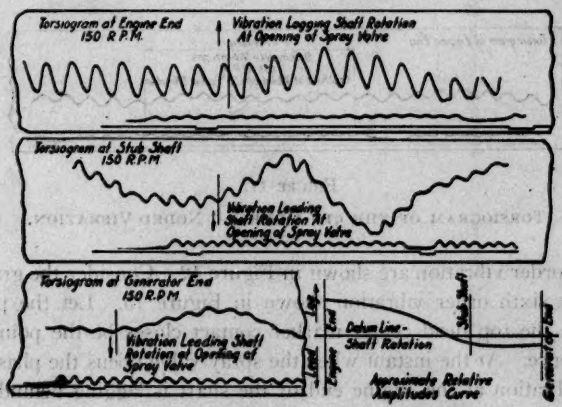
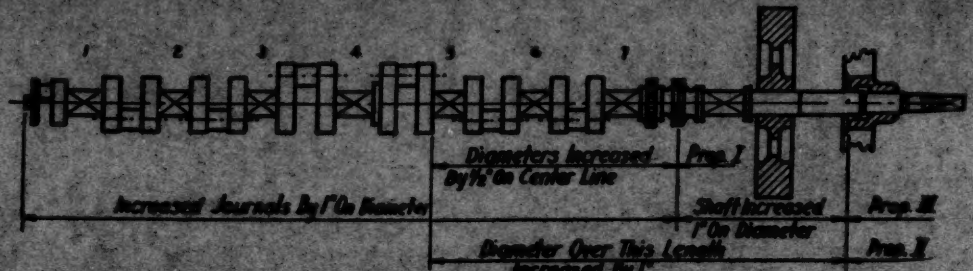


FIGURE 13.

R.P.M. is about 2.23 degrees positive or negative. This represents a total twist of 4.46 degrees from maximum positive to maximum negative. The stress set up at the after journal, due to the vibratory twist alone was calculated and found to amount to approximately 15,000 pounds per square inch for the sixth order vibration. The calculated stress neglects the effect of the oil holes. When subjected to a twisting strain, it can be shown that the material immediately surrounding the oil holes is stressed almost four times as much as shown by the calculated values.

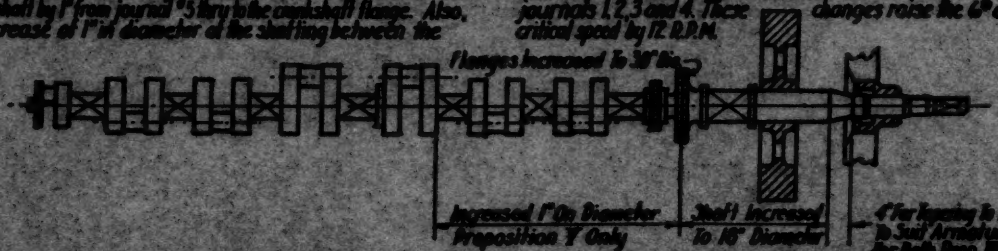
The section of crankshaft which failed was given a physical test. The material in the portion of the journal which failed showed an elastic limit of 18,750 pounds per square inch while the elastic limit of the material of the journal within the crank webb,



Proposition I. Contemplated increasing the diameter of the crankshaft by ½" from beginning of journal #5 thru to the end of flange on flywheel. These changes would raise the 6th order critical speed by 5 R.P.M.

Proposition II. It was contemplated to increase the diameter of the crankshaft by 1" from journal #5 thru to the crankshaft flange. Also, an increase of 1" in diameter of the shafting between the coupling flange and the generator armature. These changes raise the 6th order critical speed by 10 R.P.M.

Proposition III. Involves all the changes referred to in Prop. II and in addition contemplated an increase of 1" in diameter of journals 1, 2, 3 and 4. These changes raise the 6th order critical speed by 12 R.P.M.

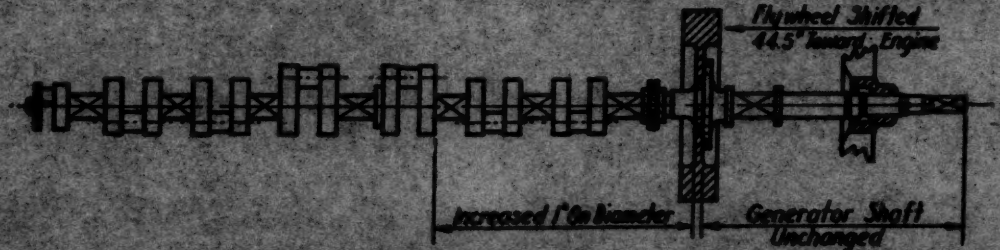

Flanges Increased To 38" Dia.
Increased 1" On Diameter Proposition V Only
Shaft Increased To 18" Diameter
4" For Keying To 15" Dia. To Suit Armature. For Both Prop. III & V.

Proposition IV. In this proposition it was contemplated to stiffen the generator shaft by increasing its diameter to 16" up to the generator armature. These changes raise the 6th order critical speed by 10 R.P.M.

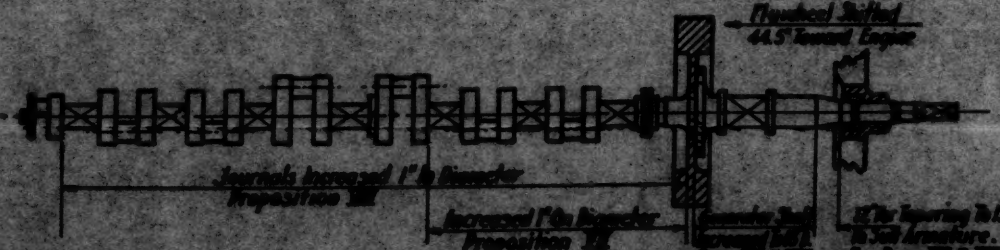
Proposition V. Changes involved in this proposition are as for Prop. III, and in addition contemplated a 1" increase in the journals 5, 6 and 7. This raises the 6th order critical speed by 16 R.P.M.

Proposition III. In this proposition it was contemplated to stiffen the generator shaft by increasing its diameter to 18" up to the generator armature. These changes raise the 6° order critical speed by 10 R.P.M.

Proposition V. Changes involved in this proposition are as for Prop. IV, and in addition contemplated a 1" increase in the journals 5, 6 and 7. This raises the 6° order critical speed by 16 R.P.M.



Proposition VI. This proposition effects a raise in the critical speed by shifting the flywheel towards the engine where its importance in vibration is reduced. This raises the 6° order critical speed 16 R.P.M.



Proposition VII. This proposition contemplated all of the changes in Prop. VI and in addition an increase of the generator shaft to 18" diameter up to the generator armature. This increases the 6° order critical speed 23 R.P.M.

Proposition VIII. This proposition involves the final changes which were recommended and for which the mathematical analysis predicted an increase of 23 R.P.M. in the 6° order critical speed. The changes involved are increasing all crank shaft journals by 1" in diameter, shifting the flywheel towards the engine about 25" and increasing the generator shafting up to the armature girder to 18" diameter.

FIGURE 14.

not unduly stressed, was found to be 23,000 pounds per square inch. The effect of the violent cyclic variation in stress on the material in the journal is evident from these figures.

The calculated stress of 15,000 pounds per square inch neglects the stress set up by the normal engine torque. For a 1000 B.H.P. engine at 150 R.P.M., the torque is 420,000 inch pounds. The maximum turning effort will be approximately twice this value or 840,000 inch pounds. The maximum fibre stress due to normal twisting alone in journals 6 and 7 is therefore 1950 pounds per square inch. The maximum stress set up by the vibration alone is almost seven times that set up by the maximum working torque. The manner in which the working torque, vibratory torque, and stresses due to bending combined, was not investigated, but it is evident from the foregoing that an excessive alternating stress was

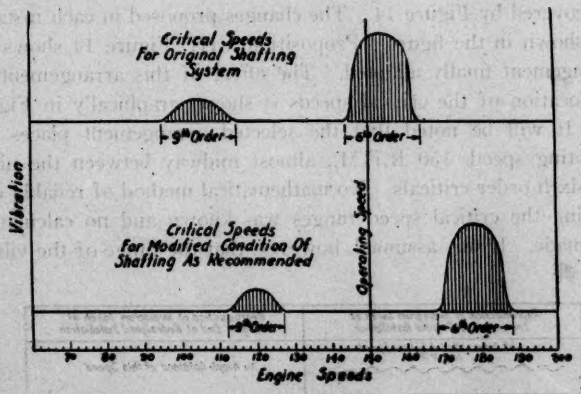


FIGURE 15.

in play. When it is remembered that this stress repeats itself more than 900 times each minute, the effect on the metal in the journal can readily be imagined.

Before any attempt was made to devise a remedy for the conditions existing, a careful calculation was made of the torsional frequency of the existing shaft. The frequency by this calculation was 942 vibrations per minute, for the one-noded vibration, checking closely with the observed frequency of 940 vibrations.

An obvious remedy for the trouble was a change in the natural frequency of the shaft system. The natural frequency of a shaft may be increased by stiffening the shaft at proper sections and may be decreased by making the shaft more flexible. Increased flexibility may be obtained by reducing the shaft section or by properly locating a flexible coupling. Reduction in size of the shaft to sufficiently lower the sixth order critical could not be effected without seriously impairing the strength of the system. Introduction of a flexible coupling would have involved the use of a device still in the experimental stage. Further, making the shaft more flexible would have tended to increase the range over which the critical extended, introducing a feature difficult to predict beforehand. Obviously, increasing the stiffness of the shaft system offered the most promise of a satisfactory solution.

Several propositions were than investigated mathematically and are covered by Figure 14. The changes proposed in each instance are shown in the figure. Proposition VIII, Figure 14 shows the arrangement finally adopted. The effect of this arrangement on the location of the critical speeds is shown graphically in Figure 15. It will be noted that the selected arrangement places the operating speed, 150 R.P.M., almost midway between the ninth and sixth order criticals. No mathematical method of reliably calculating the critical speed ranges was known and no calculation was made. It was assumed, however, that the range of the vibra-

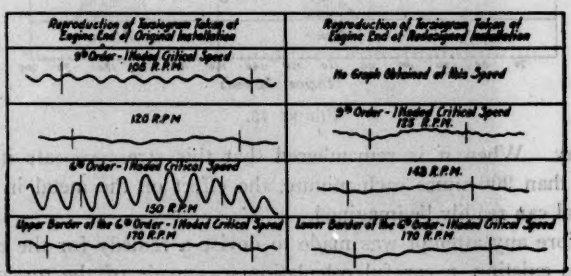
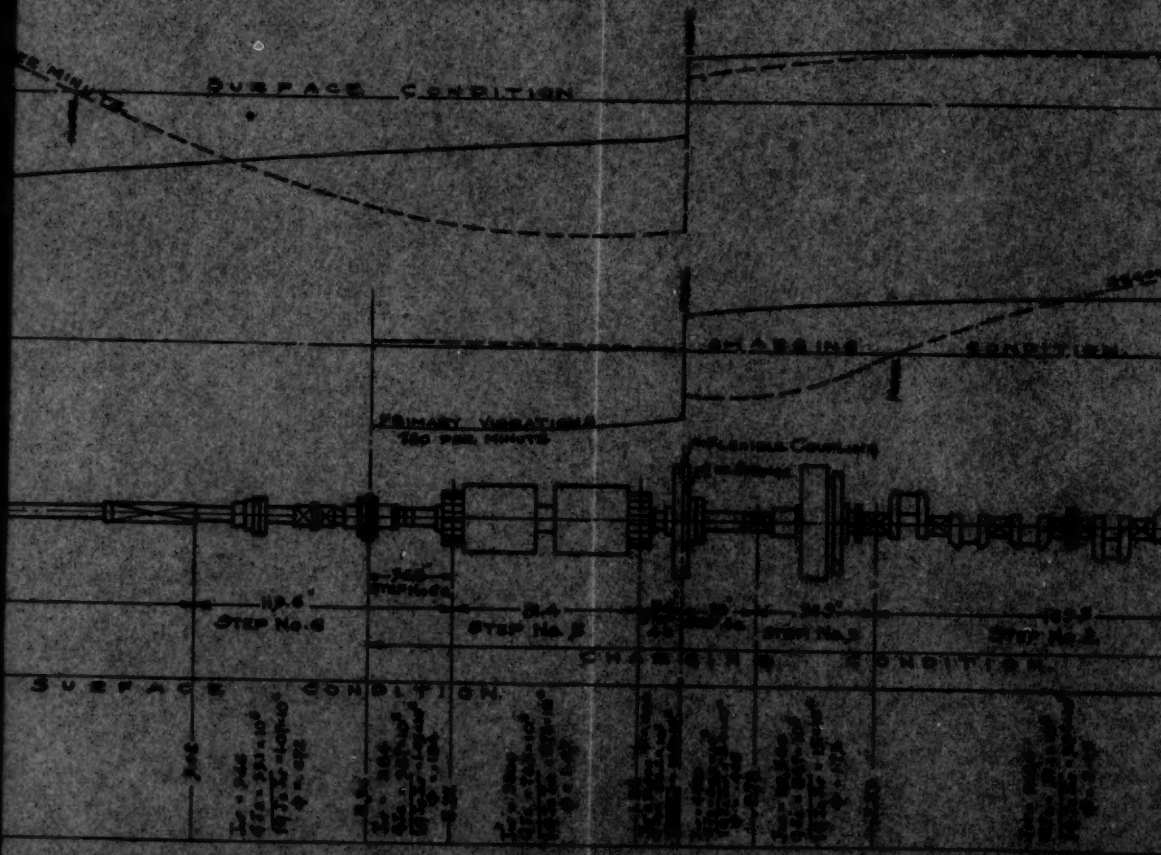

Reproduction of Torsiogram Taken at Engine End of Original Installation
Reproduction of Torsiogram Taken at Engine End of Redesigned Installation
9th Order - 1 Noded Critical Speed 105 R.P.M.
No Graph Obtained at this Speed
120 R.P.M
9th Order - 1 Noded Critical Speed 135 R.P.M.
6th Order - 1 Noded Critical Speed
150 R.P.M
143 R.P.M.
Upper Border of the 6th Order - 1 Noded Critical Speed 170 R.P.M
Lower Border of the 6th Order - 1 Noded Critical Speed 170 R.P.M

FIGURE 16.

COMPARATIVE VIBRATION FOR THE ORIGINAL AND REDESIGNED INSTALLATIONS.

CURVES OF RELATIVE AMPLITUDE.



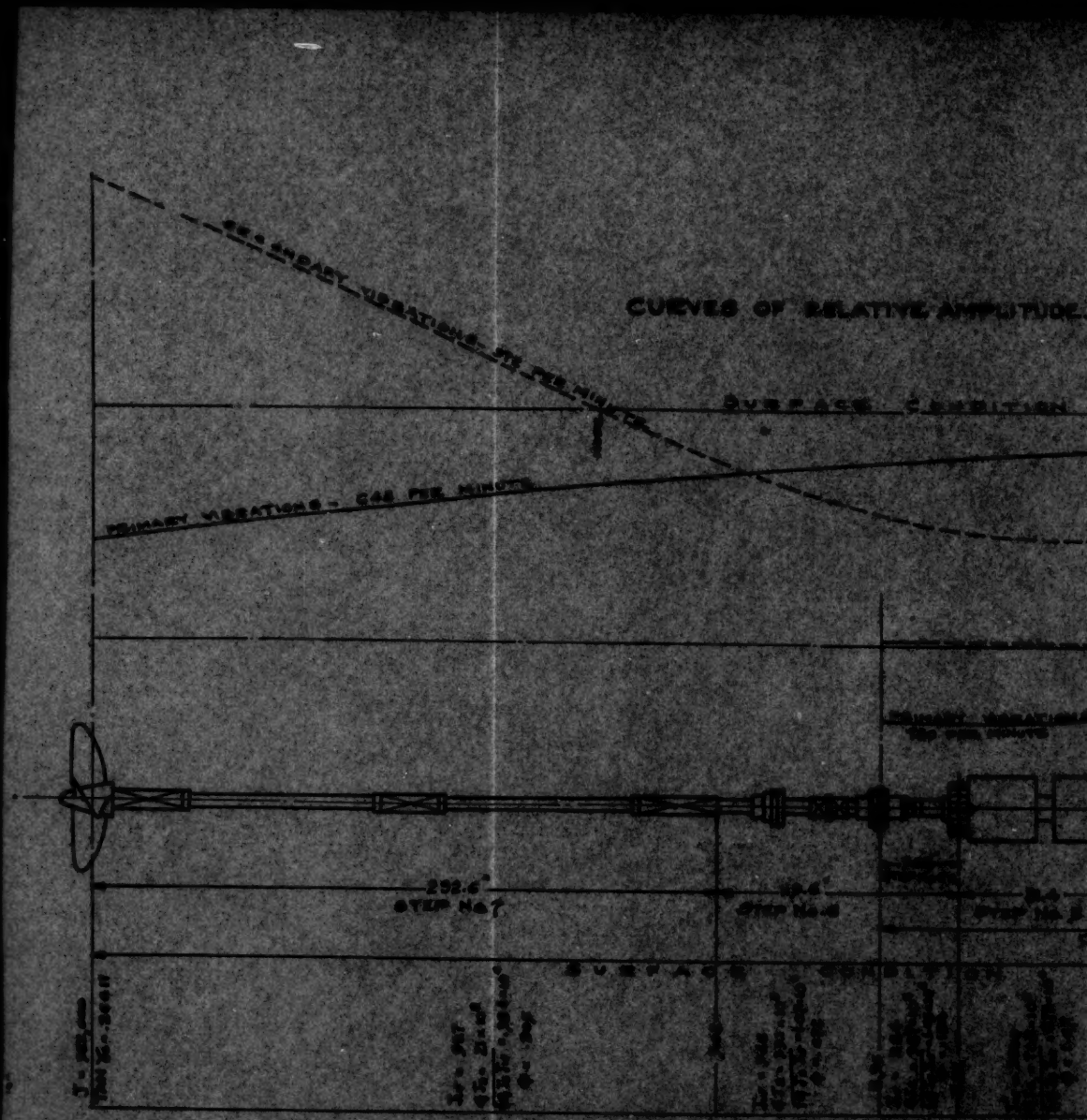
U. S. Subs. 340231

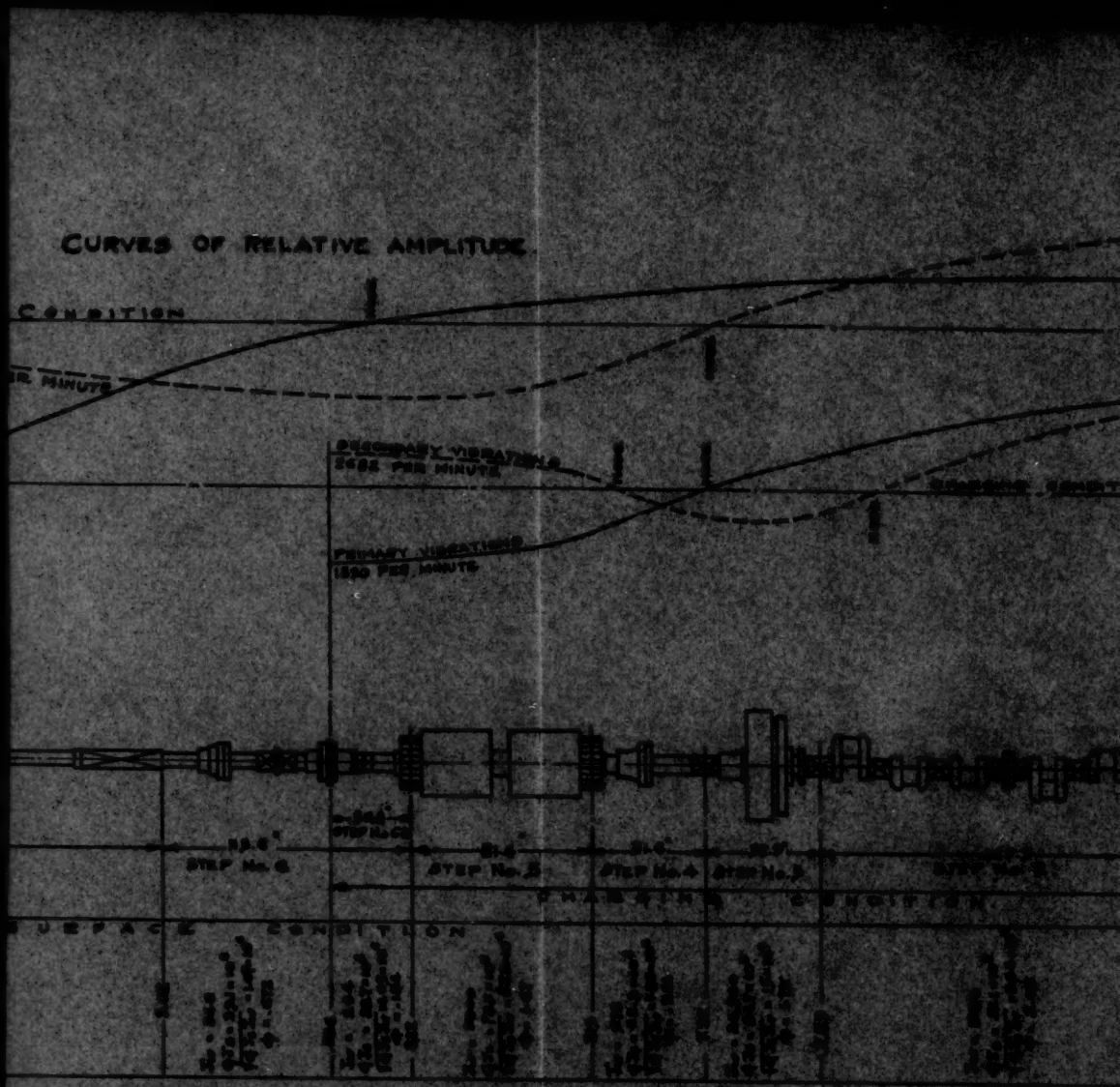
Analysis of Torsional Vibration Characteristics of Shafting System (Equipped with Flexible Motor Coupling)

All Shafts Constructed and Installed in Full Units.

Summary of Critical Speeds (RPM)				
Number of Nodes and Antinodes	Subject Condition		Condition	
	Engine Speed		Engine Speed	
0	196		196	
1				
2				
3				

Machinery Division
Navy Yard


CURVES OF RELATIVE AMPLITUDE
SURFACE CONDITION
PRIMARY VIBRATIONS = 648 PER MINUTE
252.6"
STEP No 7
SURFACE


CURVES OF RELATIVE AMPLITUDE
CONDITION
PRIMARY VIBRATIONS
1890 PER MINUTE
STEP No. 2
STEP No. 3
STEP No. 4
STEP No. 5
SURFACE CONDITION

U.S. Subs. S 48 & 51

Analysis of Torsional Vibration Characteristics of Shafting System (Equipped with Solid Motor Coupling.)

All weight constants are in lbs. units.

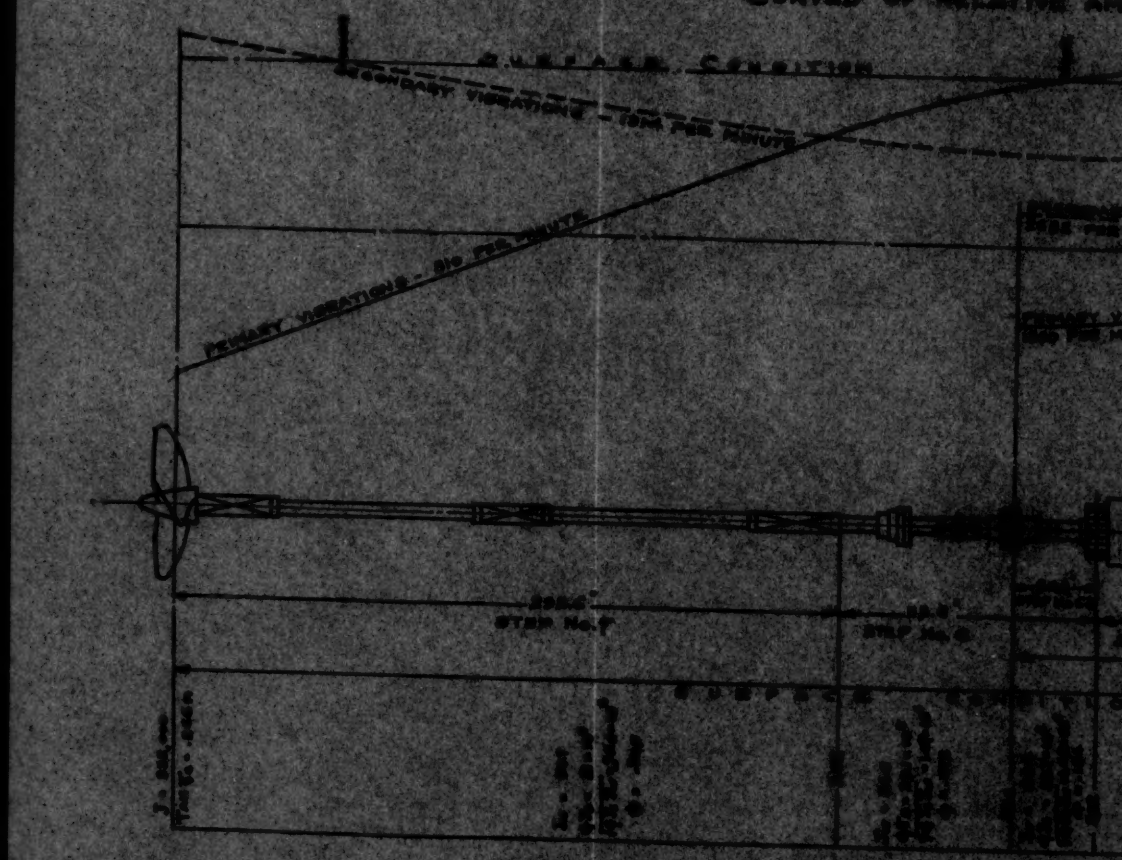
Summary of Critical Speeds

Number of Nodes and Antinodes	Frequency Constant	Critical Speed		
0	197	2[illegible]	[illegible]	[illegible]
1	34	[illegible]	[illegible]	[illegible]
1-1	-	[illegible]	[illegible]	[illegible]
2	-	[illegible]	[illegible]	[illegible]

Scale for Crankshaft Amplitudes

[illegible]

[illegible]


CURVES OF RELATIVE AM
SECONDARY VIBRATIONS - 1584 PER MINUTE
PRIMARY VIBRATIONS - 810 PER MINUTE

tion, after stiffening the shaft, would be the same as before stiffening. Any error in this assumption would lie on the side of safety.

The first dredge tested after completion of the alteration outlined above has shown an entire absence of torsional critical speeds throughout the operating range. The ninth order vibration, which was placed at 120 revolutions per minute, was found at 120 revolutions, but was so slight as to be negligible. Slight evidence of the beginning of a sixth order critical was noted at 170 revolutions per minute agreeing closely with the predicted location, see Figure 15. At 150 revolutions, the operating speed, the engine was particularly smooth in operation and free from vibration. Figure 16 shows the torsiographs taken before and after the alterations.

Another case of destructive torsional vibration, the correction of which involved the application of a torsionally elastic coupling, arose on the *S—48—51* class submarines. The vibration on the *S—48—49 and 51* was corrected by the installation of stiffer crankshafts. On the *S—50*, however, it was decided to experiment with an elastic damping coupling.

Figure 17 gives the shaft constants adapted for Prof. Lewis' method of calculation together with the critical speeds and position of the nodes as they existed in the original installation. It will be seen that the node of the primary vibrations, charging condition, is approximately midway between the nodes of the secondary vibrations. A mathematical investigation revealed the fact that the addition of a suitable amount of torsional flexibility at or near the node of the primary vibrations would not materially change the location of the secondary, two noded, critical speeds, but would greatly change the position of the single noded criticals.

Figure 18 shows the altered arrangement together with the shaft constants for the revised installation. Figure 19 shows the elastic coupling. It will be noted that the coupling provides frictional damping, the idea being to reduce the violence of the 9th and 12th order two noded critical speeds, see table Figure 18. The ninth and twelfth order 2 noded critical speeds, it was expected, would not be violent, but it was considered desirable to reduce them as much as possible by frictional damping. With the revised arrangement it was expected that there would be no serious critical speeds in the operating range except the ninth and twelfth order two

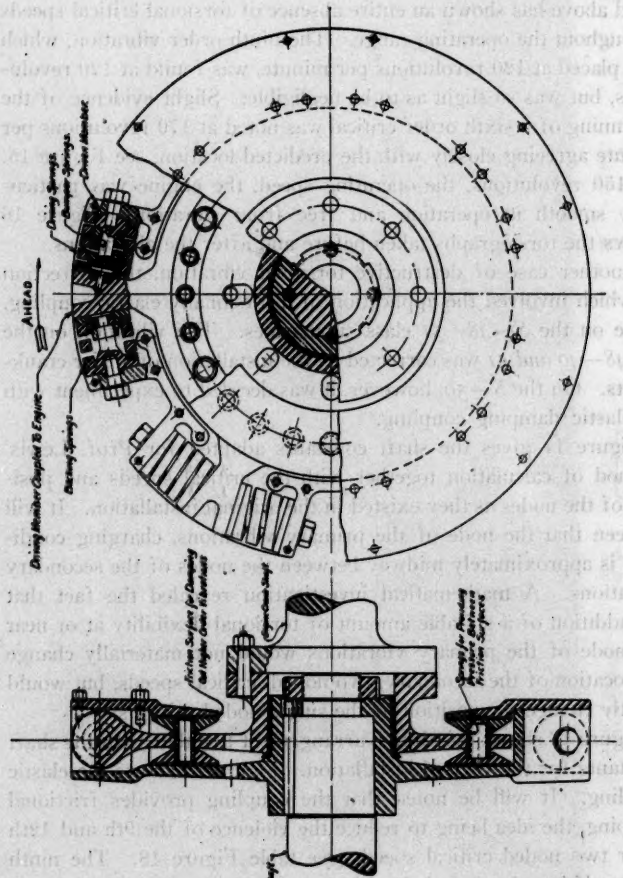
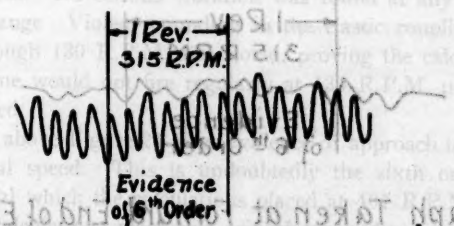
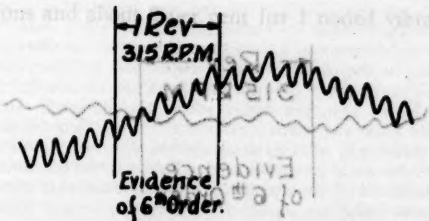

AHEAD
Driving Springs.
Damper Springs.
Driven Member.
Driving Member Coupled to Engine
Driving Springs
Friction Surface for Damping the Higher Order Vibrations
Friction Surface for Damping Pressure Between Friction Surfaces
Crankshaft
Main Shaft

Fig. 19

noded, charging condition, vibrations. It was also expected that the destructive sixth order one node vibration, present in the original installation at 315 R.P.M. Figure 17 would be located at 130 R.P.M. for the revised arrangement. As the engines were not



Graph Taken At Forward End of Engine



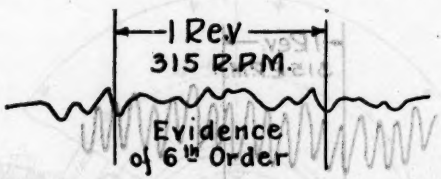
Graph Taken Aft Of Motor.

Unit Equipped With A Solid Coupling.

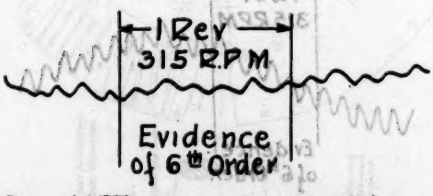
Fig. 20

capable of operating reliably below 160 R.P.M. the net result aimed for by the alterations was the elimination of all dangerous critical speeds in the operating range up to about 400 R.P.M.

Figure 20 shows the torsiograph records at 315 R.P.M. from the forward end of the engine and immediately aft of the main motor



Graph Taken at Forward End of Engine



Graph Taken Aft of Motor

Unit Equipped With Elastic Coupling.

Fig. 21

generator for the original condition. Figure 21 shows the torsiograph records for the same speed after the installation of the elastic coupling. In Figure 21 slight evidence can be seen of approach to a sixth order critical speed but the vibration is not great enough to be harmful. No serious vibration was found at any point in the speed range Violent pounding in the elastic coupling when passing through 130 R.P.M. was noted, proving the calculations. As the engine would not fire regularly at 130 R.P.M. no graphs were obtained.

As stated above Figure 21 gives evidence of approach to a sixth order critical speed. This is undoubtedly the sixth order two noded critical which the calculations placed at 407 R.P.M. Evidently the calculations place this critical too high or it covers a broad range. From experience gained with the *S—50* and subsequent installations it has been found that while the single noded vibrations can be predicted very closely an allowance of from 6 to 10 per cent below the calculated values must be made for the two noded vibrations and about 2 per cent for 1 noded vibrations.

DISCUSSION.

The Theory of the Impeller (February, 1926). A. HOLLANDER, M. E., Byron Jackson Pump Mfg. Co. Taking up an article about the theory of the impeller, meaning in this case the theory of centrifugal pumps, the first question is, how the new theory compares with the present one, which may be found in any handbook, and is also given in the Appendix of the present article. Does the author refute the old theory or only amplify it from some new viewpoint? A refutation is not possible, if, as in this article, the fundamental idealizing assumptions are the same as for the old theory, the deduction of which is of course correct. Realizing this, the author considers the old formulas valid, as he states in his final paragraph that it would be interesting and fruitful to compare his results with the old ones. His intention seems to be an amplification from the viewpoint of what he calls the "centrifugal effects in the impeller," which according to his research never have been disclosed.

Unfortunately, the forces acting on a fluid particle moving in a channel, which has a uniform rotation, are quite different from the ones disclosed by our author. They were first given and the fundamental equations deduced from them by Euler in 1754 and may be found in a number of books dealing with hydraulics or water turbines.* They lead to the accepted fundamental equations and I consider their repetition here unnecessary, but call attention to the specially clear exposition in Prof. Escher's "Water Turbine." The pungent footnote at the close of his deductions is worth quoting:—"There are even today men, who seriously believe that work can be gained from the centrifugal force. They forget that the centrifugal force, as a reaction to inertia, is a *result* of the rotary motion and therefore can not retro-act so as to advance or retard (fördernd) this rotation. One involuntarily thinks of the Baron of Munchhausen who wanted to lift himself by his own hair."

Our author introduces a centrifugal force p_c first as *pressure* (page 21). On the next page he makes it *force*, to change it to *pressure* on the next page again. (This promiscuous mixing of terms continues throughout. The term *pressure* is later applied to expressions like $\frac{v^2}{2g}$, that is, head and where it does not lead to errors it is annoying.) The centrifugal force being a radial force can not have any moment about the center of rotation, but the article, to get a moment, calls this force a pressure, and reducing it in the proportion of areas takes its component only in the direction of the relative flow and disregards the other component, which acts on the channel. Such errors and the omission of a number of forces acting on the fluid particle, as mentioned before, lead to the surprising result that the total head produced by the pump—

$H = \frac{\Omega^2}{K_\omega^2}$ where Ω is the tangential component of the absolute velocity

* E.g. Cammerer "Wasserkraftmaschinen." Banki "Energie-Umwandlungen in Flüssigkeiten." Escher-Dubs "Die Theorie der Wasserturbine."

at the outlet (called the velocity of entrainment) and K_ω a function of the ratio inlet and outlet diameters of the impeller in case of constant radial velocity, and a further function of the ratio of radial velocities in case they are changing from inlet to outlet.

Furthermore, the head at any radius R with the assumption that it increases in a straight line from inlet to outlet is also—

$H = \frac{\omega^2}{K_\omega^2}$ where ω is the tangential component of the absolute velocity at radius R and K_ω a function of the ratio radius R and the outlet diameter in case of constant radial velocity or a further function of the radial velocity ratio, if that is variable.

The lack of any coefficients would be a very welcome innovation if the difference between the idealized assumptions and actual flow would not require something to take care of the friction in the impeller channels, or the losses at the entrance and the losses connected with the transformation of the great kinetic energy into pressure at the outlet of the impeller. One would think that possibly the losses in the impeller channel are taken care of by a reduced ω (bearing in mind that following the author's designation this is not angular velocity but the tangential component of the absolute velocity). However, this is not the case, as in the examples given the velocity diagrams are derived from the actual vane angles without any modification. It is natural, therefore, that the conclusions lead to results contradictory to experience.

Taking for instance the author's figures for (page 32) and figuring the head for two impellers, one with an outlet to inlet diameter ratio of 1.8 and another with a ratio of 4, we find that the latter impeller produces 43 per cent more head than the former while running with the same speed and having the same diameters and outlet angles. Our experience is that the head, depending on the case design, will not be materially different. Or, taking the second interpretation of the final formula, according to which the total head is made up of a centrifugal and kinetic head (called sometimes kinetic pressure), the latter amounting to over half the total, the author seems to conclude that because there are pumps with 80 per cent efficiency and because the losses due to disc friction, leakage, bearing friction, and "the little friction of the delivered water" leave practically nothing for other losses that there is no loss in the impeller channels and that the diffusors are of no use and the volute form is of no importance.

Actually, the 80 per cent efficient pumps have carefully laid out volutes and not a "concentric annulus" as he suggests for the last stage multistage pumps, while the lower stages of multistage pumps are similarly provided either with volutes or properly formed diffusor passages to take care of the transformation of kinetic energy into pressure.

The disc friction in the table is figured very high, in fact, the first formula from which the rest are derived (on page 89 and repeated on page 98) gives a value ten times lower than the one on the following pages, which had been applied in the examples. Unwin, Daugherty, and others give values between the two.

However, if we disregard the incorrect deductions and consider only the vane shape arrived at, it is possible that this will give good results under certain conditions.

Proceeding with the author by assuming a shut-off head H_s in per cent of the rated head and figuring the peripherial velocity from this $v_0 = \sqrt{2g\,H_s}$,

the impeller diameter is determined if the R.P.M. and the inlet diameter of the impeller are given, and the whole vane shape may be figured and drawn up. In the few examples which I tried to check, there was an undesirably sharp rise of the relative vane angle a near the inlet, but further checking would be necessary to ascertain whether this is a characteristic of the equations or not. However, simpler equations like a plain logarithmic curve or a modified one give results easily comparable with the ones given in the article.

Some of the suggestions, as the emphasis on the suction end of impeller are useful, others not practical. For instance, that the formula for maximum speed of a single suction impeller—

$$N = 420 \frac{Q}{D_p^2} \text{ where } \frac{Q}{D_p^2} = \frac{c_p}{.408} \quad c_p = 10$$

gives $\frac{Q}{D_p^2} = 24.5$, a maximum speed of 10,000 r. p. m., irrespective of head—certainly high enough.

In practice, the limiting value of this speed is dependent on the specific speed of the pump, a term or factor which determines the type of the impeller. With high specific speeds, we get what the author calls "shallow impellers" and the standard practice is to use scoop arm or Francis wheel type impellers, which will give good efficiencies and eliminate the troubles encountered with cylindrical vanes.

Another suggestion elaborately presented is how to get a performance curve with a non-overloading brake horsepower characteristics. The author's suggestion, a decreasing radial velocity, is far from desirable, being simply throttling at the suction or entrance of the impeller. A good high suction lift will do the same, sometimes against the wishes of the designer. For pumps with lower lifts, a non-overloading BHP curve is easily attained with constant radial velocity and the brake horsepower curve made flatter and its maximum nearer to the best efficiency point than the one presented by the author, by choosing small outlet angles and relatively large impellers with high shut-off heads. One does not have to be scared then from what the author improperly calls "the post-normal part" of the characteristic curve (a term generally used in place of *pressure volume curve*), which has just as much efficient range as the one called *normal* in the article.

Taking up a more practical suggestion,—in reference to labyrinth rings, our experience was just the opposite of the one described. We find that they are excellent for clear water, but we would not use them for gritty water and we are surprised that they outwore the straight rings three to one in the steel mills. The specifications of users invariably call for straight rings in case of gritty water and a number of labyrinth rings had to be replaced, because they did not last at all, by straight rings when the fluid pumped contained sand.

As a conclusion, it may be stated that so far as the theory of centrifugal pumps is concerned, what we have is nothing to be proud of and according to Prof. Thoma does not deserve the name of theory. The trouble is with the assumption of an infinite number of vanes to get the same velocities and pressure on a circle. The new theories based on the circulation theory and experiments with aerofoils and propellers are in their infancy.* They have already given good results with propellers in infinite space, as on a boat or aeroplane, and are being applied to propeller turbines.†

* Lamb "Hydrodynamics" Reports of the National Committee for Aeronautics Dr. Kármán "Lur Theorie der Luftschraube" Ld.v.d.I. Dec. 1924.

† Dr. Eck "Hydrodynamic Theory of Turbines and Centrifugal Pumps"—London Engineering Jan. 22, 1926.

Their main advantage will be a truer picture of the velocities and pressures and the whole flow through the pump in place of an imaginary flow greatly modified by coefficients, which do not make the picture clearer. We may have known the resistance of aeroplane wings before the late theories, but new shapes without the knowledge of the mechanism of the flow could neither be developed, still less figured in advance. Until the similar but far more complicated field of pumps and turbines is made accessible, it is suggested that the standard American handbooks, as, Greene, Daugherty, and Lowenstein be used, possibly with the latest modifications given by Pfleiderer (up to the present available only in German).

Incurring the danger of its being used against me, I can not abstain from closing with a quotation from the very interesting historical review of the article commenting upon Papin's explanation of his pump:—"However, in regards to this matter, it behooves us to be very modest, since the centrifugal pump of today is very far from being correctly understood, even by its most reputed makers."

High Temperature Insulation (February, 1926). J. M. WEAVER, Keasby and Mattison Company. The timely and valuable article on High Temperature Insulation by Lieut.-Comdr. C. S. Gillette, published in the February, 1926, issue of the JOURNAL OF AMERICAN SOCIETY OF NAVAL ENGINEERS, presents a number of subjects on which a discussion may be of interest to the members of the A. S. N. E. The following comments are presented from the point of view of a writer whose experience has been obtained in the manufacture of insulating materials, in the application of such materials, and in a study of them under service conditions.

HAIRFELT.

The use of hairfelt as an insulating material should be upon cold surfaces, to avoid freezing or to hold low temperatures in refrigeration. Hairfelt probably would not be injured at temperatures ranging from 100 degrees F. to 119 degrees F., as mentioned on page 7 of the JOURNAL, but the danger when used upon a warm surface is that the temperature range would probably be hard to control to a limit of 119 degrees F., and a temperature very little higher than that mentioned would produce a disagreeable odor and seriously impair the insulating material.

A warm pipe having a shape or location that might suggest a preference for the use of hairfelt can be insulated effectively by wrapping with asbestos rope. In places where abrasion may occur, asbestos rope can be protected by a coating of asbestos cement finished with a canvas jacket. Asbestos rope that is twisted loosely has a higher insulating value than tightly twisted rope. A layer of loosely twisted next to pipe, finished with a layer of tightly twisted is a good combination.

Tightly twisted asbestos rope finished with a coating of fire-resisting cement has been used for many years successfully, on blow-off pipes of certain types of boilers.

Rope insulation is in common use on European ships, on some of which the rope is finished with a coating of paint only.

ASBESTOS.

JOURNAL, page 16, last paragraph:

"The life of asbestos fiber decreases at temperatures at which the water of crystallization is lost. The water of crystallization is lost by the fiber

at comparatively low temperatures, but it is not until a temperature of 400 degrees or 500 degrees F. is reached that the loss has a serious effect in shortening the life of the insulation." This statement may mean to the average reader that asbestos fiber would be seriously injured when serving as a bond in pipe covering subjected to temperatures of 400 degrees to 500 degrees F. Most emphatically NO, for magnesia and diatomaceous earth insulations, made with Canadian fiber.

Chrysotile asbestos from the Thetford Mines district in Canada loses but one or two per cent of its water of crystallization at 400 degrees or 500 degrees F., and changes but slightly in strength up to 750 degrees F., therefore showing that there is little additional loss of water of crystallization, which is the strength-giving quality in fiber. Not until a temperature between 1000 degrees and 1150 degrees F. is reached will all the combined water of crystallization be driven from chrysotile fiber.

The asbestos fiber in insulation used upon a pipe having a temperature of, say, 800 degrees F., will not be subjected to that temperature. The surface of the insulation in contact with the pipe is the only part of the insulation that will have a temperature of 800 degrees F. The outside surface of the insulation will have a temperature so low that one's hand may be held upon it. The mean temperature of the insulation will be hundreds of degrees less than the pipe temperature, and the asbestos fiber which bonds the covering will not be injuriously affected by the heat.

Asbestos is a general term used for several kinds of fibrous minerals, of which the principal varieties are:

Chrysotile—Composed chiefly of silica and magnesia. The Canadian variety. Green before and white after fiberizing.

Crocidolite—Composed chiefly of silica and iron. An African variety. Crocidolite is blue in the crude rock and also after fiberizing.

Amosite—Very close in analysis to Crocidolite—silica and iron—but very different in appearance. Amosite is brown and harsh.

The different varieties of asbestos differ in their heat resistance. Chrysotile has the greatest resistance to heat. But chrysotile from different localities shows variations in heat resistance. A comparative test made on chrysotile fibers from Canada and Arizona, at a temperature which merely made the Canadian fiber glow, caused the Arizona fiber to fuse into a bead. An ordinary chemical analysis of these two lots of fiber might have shown practically the same composition. But a searching analysis would disclose other elements sufficient to account for the difference in refractory quality. A very slight amount of soda may have been responsible for the fluxing of the Arizona fiber. Or perhaps a little lime might be responsible. Silica is highly refractory. Lime is very heat resistant; but when the two are mixed in proper proportions they form a flux. Some elements change their heat resistance in the presence of other elements. For example: fusible links are composed of metals which have higher individual melting points than the alloy they form.

Crocidolite asbestos, the blue variety, may have been the source of the information given by Lieut.-Comdr. Gillette, with reference to material change in strength at temperatures of 400 degrees or 500 degrees F. Such information could not apply to the chrysotile variety, whether from Canada, Arizona, or any other place.

The following quotation regarding blue asbestos is from a publication issued by the Canadian Department of Mines:

"As compared with 'chrysotile,' it possesses remarkable tensile strength; but it is different in fire resisting qualities, and for that reason its substitution for chrysotile—which was at one time attempted—was a complete

failure. While good asbestos fiber contains almost 80 per cent silicate of magnesia, and only from 1 to 3 per cent oxide of iron; crocidolite contains only 50 per cent silica, and not less than 34 to 48 per cent oxide of iron. A number of experiments were made with crocidolite several years ago, by Mr. Alfred Fischer, General Manager of the Bell's United Asbestos Company, 'Limited', London, with a view to utilizing the African fiber; but the results were very unsatisfactory; and it was stated at the time that the fiber was unsuitable for engineering purposes since it would not stand much heat without disintegrating and becoming quite rotten, this negative result probably being due to the fact that a portion of the iron disclosed by analysis was in the form of a ferrous salt. It is further stated that when exposed to air and heat, this salt oxidizes, and alters the composition of the asbestos to such an extent that it is easily charred."

Lieut.-Comdr. Gillette has said of asbestos: "While it is heat resistant, it is not, in most of its commercial forms, a good insulator."

Asbestos is not a good insulator in any of its commercial forms—for high temperature insulation, to which I understand the remark applies. But pipe covering composed principally of asbestos, in quite a number of construction forms, furnishes very excellent insulation on a wide range of temperatures from hot water to high pressure and superheated steam.

Asbestos in its crude state will conduct heat readily. It does not become an insulating material until fiberized. Canadian asbestos can be divided into fibers so fine that they become almost invisible to the naked eye. The more finely the fiberizing, the higher the insulating value of the asbestos. Why? Because the asbestos itself does not do the insulating work, but the air it entraps is the barrier to the flow of heat. The highest resistance to the transmission of heat is found in those insulating materials in which the air forms the largest volume and is divided into the greatest number of cells, as in magnesia and diatomaceous earth coverings.

Asbestos is a stone. Magnesia is a stone. Diatoms are stone. Step on a stone floor with bare feet and it feels cold because heat from the feet flows rapidly to the stone. Transform asbestos, magnesia, and diatoms into solid stones, and it is probable they would conduct heat at the same rate. The relative insulating value of these materials depends upon the comparative number, size and volume of the air cells that are confined by them in their finely divided state.

Asbestos is a heat resistant fiber that can be made into various forms to confine air, which is the insulating medium. Crystals of carbonate of magnesia have shapes that make them touch with very small points of contact and entrap much air. Diatoms are very well described in Lieut.-Comdr. Gillette's article.

Asbestos enters into practically all forms of heat insulators. The quantity in 85 per cent magnesia covering should be not more than 15 per cent, as that has been found by manufacturing experience to be the proper proportion. In other words, each hundred pounds of the mixture from which 85 per cent magnesia covering is made should contain 85 pounds or more of carbonate of magnesia and not to exceed 15 pounds of asbestos fiber. The more finely the asbestos fibers are divided and the more even their distribution throughout the mass, the stronger, lighter, and more efficient will the insulation be.

CALCINED MAGNESIA PIPE COVERING.

For many, many years 85 per cent magnesia pipe covering has been greatly maligned with regard to calcination. There is a widespread belief

fostered by sales efforts on behalf of other types of pipe covering that the calcination of magnesia pipe covering results in the immediate destruction of the covering. As a matter of fact, when 85 per cent magnesia insulation is applied upon a pipe in which the temperature is high enough to calcine carbonate of magnesia, thereby changing it to oxide of magnesia, the pipe covering is made more efficient as an insulator. At a temperature of, say, 800 degrees F., calcination would at first take place only on the surface of the pipe, and the increased efficiency of the insulation would retard the calcination into the body of the covering.

When the covering is bonded with a thorough intermixing of fine asbestos fiber, the calcined magnesia is held in place and the covering continues to perform its work over a long period of time.

The Mellon Institute of Industrial Research investigated, in 1921, the serviceability of 85 per cent magnesia insulation for superheated steam surfaces. The following quotation is made from the report of that work:

"The piping in the Buffalo General Electric Company's plant is covered almost exclusively with 85 per cent magnesia from about 2 inches to 4 inches in thickness, the thickness depending upon the size of the pipes. The temperature of the steam ranged from 600 degrees to 700 degrees F. with peaks of 800 degrees F.

"The coverings were thoroughly examined, particular attention being given to general appearance, fit on pipes, sagging or powdering, calcination and mechanical strength.

"Representative sections were removed from 3 inch, 6 inch, and 10 inch pipes and shipped to the Mellon Institute of Industrial Research, Pittsburgh, Pennsylvania, for further laboratory investigation.

"The coverings presented a very fine appearance and were in practically the same condition as when they were installed. Because of calcination, in some cases the coverings were slightly softened at the inner surface, but *there was no tendency to powder or sag.* The coverings were in such excellent physical condition that no difficulty was encountered in removing them from the pipes and shipping from Buffalo to the Mellon Institute. They arrived at the Institute in as good condition as they were in at the time of removal from the pipes at the plant.

"For approximately six months there has been in operation at the Mellon Institute an experimental still covered with 85 per cent magnesia blocks. After this still had been in operation for a few days a fire occurred with the result that the 85 per cent magnesia block insulation was completely soaked with water in the process of extinguishing the fire. During the six months period of operation there had been approximately twenty-nine heats. Surface temperature measurements of the still showed maximum temperatures of 1200 degrees to 1300 degrees F. to each heat, and these maximum temperatures lasted approximately 28 hours during each run. One of the 85 per cent magnesia blocks was removed from this still in order to determine its mechanical condition under the severe service.

"The sample block showed surprising strength in view of the temperatures of 1200 degrees to 1300 degrees F. to which it had been subjected. The block had assumed a pink color throughout, probably due to the presence of iron in the asbestos fiber binder. Mechanically, it was in good condition and perfectly serviceable for a new installation. A man weighing 165 pounds standing upon a 6-inch square piece caused practically no compression."

It is pertinent to say that 85 per cent magnesia coverings as made by different manufacturers, differ in their fiber content, in the fineness of the fiber, and in uniform distribution of the fiber, and in the degree of purity

in the magnesium carbonate. The 85 per cent magnesia on which the Mellon Institute report was based was bonded with Canadian chrysotile asbestos fiber that was carded to open up and separate the fibers to their utmost fineness.

Consequently, in view of the differences in construction that may be found in the magnesia covering of various brands, the engineering profession has been justified in demanding, for use on high temperatures that are becoming more general, a type of insulation having high insulating efficiency and the utmost possible resistance to disintegration. This demand has been met satisfactorily by the production of an insulation composed of diatomaceous earth, so very well described by Lieut.-Comdr. Gillette. His recommendations to use diatomaceous earth coverings at temperatures beginning at 501 degrees F. will prove to be good engineering practice.

NOTES.

EXPERIENCES WITH MOTORSHIPS.

By Chief Engineer Bleicken of the Hamburg-America Line. Translated by E. C. Magdeburger.

The question of steamship *vs.* motorship is being disputed today as it was 15 years ago when the first motorship was put into service. A general solution of it is impossible and each individual case must be decided independently. The fact that the steamship has an incomparably larger experience in back of it should neither be overlooked nor underrated. Here the time has long established the healthy interchange of experiences which often purposely is being denied to the motorships. It can hardly be doubted that this is not beneficial to shipping and there seems to me no gain in it, even to the builders, when each must pass through the same sad experiences, which are unfortunately inevitable. The following is a retrospective view of the joys and hardships gone through with merchant marine motorships in the last 12 years, with the experiences of the Hamburg-America Line serving as foundation.

The first motorship was bought in 1912; it was the twin screw *Christian X*, equipped with Burmeister & Wain four cycle single acting engines of 1250 indicated horsepower. The ship made eight long cruises to America resulting in important experiences. Fuel and exhaust valves had to be renewed with stereotyped regularity and after a year or so the cylinder jackets one after the other developed cracks. Valve failures occurred mostly when full speed was resumed after a short stop. The fuel valves, due to slow speed of the engine, would be kept open longer, the flame entered the interior and the valve would be ruined. The same happened when going against the seas in a storm, so that on the third trip, after all the spare valves have been used up, it became necessary to reverse the course and enter the nearest port. The real cause of these troubles was lack of experience; they were only childish diseases. Sticking of the fuel valve resulted from unsuitable packing. In addition to this, there was as yet no experience in handling different varieties of fuel oil, which always require minor adjustments when changing from one to another. Excessive use of lubricating oil, which burns at high temperature and accomplishes the exact opposite from what it was intended to do, resulting in sticking of the fuel needles. Shredded lead proved to be an excellent packing for this purpose. Exhaust valve troubles were solved in a similar manner by changing the construction of the valve cage to include a separate valve seat ring which keeps the valves tight quite satisfactorily. If the fuel contains sulphur and even the smallest amount of free water the exhaust valves suffer considerably as has been proven by experience, whereas without the water in the fuel they are not affected. Filters or fine mesh strainers not only separate dirt and other foreign substances but also prevent water from getting into the cylinders and thus protect the exhaust valves. The cracking of cylinder

jackets was traced to excessively tight-fitting cylinder liners and nowadays such cracks from overstrain do not occur on the ships in service. Improvements in cooling water circulation were also introduced.

The war interrupted this development, although priceless experience with high-speed oil engines was obtained, which later could be utilized despite the difference in requirements of merchant marine service. Since a large number of high-powered submarine engines were awaiting installation at the end of the war the Hamburg-America Line decided to use them in the ships for its East-Asiatic service where oil at cheap prices could be secured. These high-speed engines could only be used when coupled to a gear drive, which at the time was like a leap in the dark. It is a pleasure to record that the practice met with excellent success.

The engine speed of 230 R.P.M. resulted from limiting the mean indicated pressure to 102 pounds per square inch when developing the maximum required shaft horsepower of 3300 (1650 S.H.P. per shaft) in the 10 cylinders of 20.866-inch bore and 20.866-inch stroke. The R.P.M. of the propeller could be varied freely and in order to get the best propeller efficiency a speed of 85 R.P.M. was selected. The required speed of 12 knots was easily attained. Due to constancy of torque produced by oil engines a one-half knot higher speed can always be assumed in comparison with coal-fired ships of equal engine power.

Navigation of these ships is also quite free from objections. Originally they did not steer so well in the Suez Canal but an increase in rudder area improved this condition. For a new ship a different type of stern post and rudder would probably be chosen to obtain better results.

The comparatively small engine cylinders (20.866-inch diameter) require very little air when maneuvering and shortage of maneuvering air even in such difficult waters as the mouth of the Scheld has never been experienced. On the other hand, however, it was difficult to go suddenly from full ahead to full astern when the ship has already acquired high speed. The heavy flywheel installed between the engine and the pinion shaft could not be stopped quickly enough when running at 230 R.P.M. Later on a pneumatically operated brake was installed, which was so connected to the reversing mechanism that in *stop* position of the starting (reversing) lever the brake was applied to the flywheel.

The temperature of exhaust gases at full speed of the ship varies between 750 and 850 degrees F. and exhaust valves have suffered frequently because they were salt water cooled, with the ever present possibility of salt deposits leading to burning up the valves. Uncooled exhaust valves are gradually replacing the older type with satisfactory results. Sticking of fuel valve needles was not observed with this type of engines because shredded lead was used as packing material from the beginning. The air starting valves had all to be replaced, not due to the construction of the valve itself but because chambers in the cylinder heads constituted part of the air starting manifold. Experience has shown that these chambers could not be depended upon to be free from lubricating oil, resulting in a number of heavy explosions with the inevitable breakage of cylinder heads and starting air manifold, despite relief valves and other precautionary measures. These difficulties were overcome by redesigning these valves, the interior of the valve cage connecting direct to starting air manifold, care being taken that all trace of lubricating oil is blown into the cylinder as soon as the valve opens, so that no accumulations worth mentioning can exist.

The pistons are made in two parts in order to be able to renew the lower part which alone is subject to wear. The upper part is better made of forged steel, which proved quite successful in service. The cast iron upper

parts on the motorships of the Hamburg-South America Line have also been replaced by new ones made of forged steel. Piston rings must be inspected every 12 months and the life of the two upper ones, which are exposed to the most wear, is usually about 12 to 18 months. The rings are ordinary snap rings. Experiments are being carried on with so-called Eclipse piston rings which have given very satisfactory service on other motorships.

Scraper rings must be inspected as often as possible since they are largely responsible for excessive consumption of lubricating oil. With trunk piston type of engines the oil consumption is always high and in the tropics where the oil becomes very thin this is especially noticeable. With worn-out scraper rings large quantities of lubricating oil are lost into the cylinder, which at the high prices of oil becomes an appreciable cost factor. Intensive cooling of lubricating oil helps somewhat but the condition of scraper rings is far more important.

Working pistons on the first two ships (*Havelland* and *Münsterland*) were cooled by oil, without separating piston cooling oil from that used for lubrication. On later ships fresh water was used for that purpose, whereas on the *Monte Sarmiento* oil cooling was again resorted to, but this time a separate system was provided for piston cooling oil. Experience with the first two ships showed that oil could not continuously be kept clean enough in service. Should sea water get into the oil in some way or another, the oil thickens considerably with the inevitable danger of insufficient cooling of the pistons. A better guiding of cooling oil in the piston could no doubt improve piston cooling materially. It must also be mentioned that despite the very thick oil used at times no casualties with pistons resulted, although these were always expected and feared. Under these circumstances it was decided to use a more suitable cooling medium—fresh water, although it meant such costly complications as additional water cooler and circulating water pumps, etc. Opinions differ on the experiences with fresh water piston cooling to date. First of all it has been shown by actual operation that it is impossible to keep water entirely free from oil. Telescopic pipes that were later installed with the pipes plunging into stationary holder inside the crank case carry up on them a remarkably large amount of oil despite the packing used to prevent it and a wide variety of these packings were tested without any improvement. But oil, of course, can be easily separated from water either by standing in tanks or centrifugally. Much more important are the casualties to the pipes leading from telescopic pipes to the piston crown. These would burst frequently and up to date this difficulty has not been entirely overcome. The impacts occurring in these pipes can become quite large. That these are attributable to hydraulic hammer seems to me shown by the fact that the copper pipes originally used would show the characteristic swellings prior to their destruction. Enlargements serving as air chambers as well as avoidance of sharp turns in the piping together with change to steel in the material resulted in encouraging improvements and it is hoped will lead to ultimate success if carried out further along the line indicated. Accurate tests during shop trials of an engine have demonstrated that air chambers arranged at the telescopic pipes themselves are not sufficient, despite snifting of large amounts of air, to prevent hammer in the cooling oil piping—oil was used in that case. By installing in the piping a large air chamber, equipped with oil level gage and connecting its air space to the air system, a perfectly hammerless operation was achieved and the pressure maintained constant at 35 pounds per square inch.

Vogtland was operated for the last year and a half with fresh water without any large casualties to its piston cooling pipes in the last six months. Neither had *Friesland* any casualties to report to date. The addition of soda to the circulating water in order to obtain in the pipes a protective layer against corrosion has shown so many disadvantages that the practice has been completely abandoned, because in time some circulating water gets into the lubricating oil and destroys its lubricating properties so badly that the whole installation is jeopardized. Opinions on the correct way of cooling pistons of the trunk type differ very widely as mentioned before. Although the Hamburg-South America Line abandoned fresh water cooling, the Hamburg-America Line uses it even on its most modern motorships. However it must be added that whereas *Monte Sarmiento* of the H.-S. A. L. employs engines of larger cylinder bore, with higher piston speed and even a supercharger to obtain substantial increase in power when necessary, the ships of the Hamburg-America Line have not used any such as yet.

Cylinder bores of the ex-submarine engines of the Hamburg-America Line ships show to date no appreciable wear after four years of service. It has been known, however, that *Monte Sarmiento* suffered severely from piston seizures. The reason for it is most likely a combination of circumstances. The fact that both rings and piston bodies suffered seizure evidently indicates excessively high cylinder wall temperatures. This temperature is a function of wall thickness which in turn depends upon the diameter of the cylinder bore. Furthermore the pre-compression of air in the supercharger also increases the cylinder temperature. On the other hand, after changing to oil for piston cooling no more difficulties have been observed. The mean indicated pressure has been slightly reduced, however, at the same time. This leads to the conclusion that lubrication was at fault, either through insufficient oil on the rubbing surfaces or destruction of lubricating qualities of the oil. The material of pistons and piston rings or inaccurate assembly could also be the causes of trouble. The design of the piston was changed later on, but this does not mean that it was suspected as one of the main causes of trouble. It must apparently be assumed that a number of more or less important factors served as causes of these difficulties. Nevertheless the modifications undertaken resulted in perfect operation of this large installation and the experience is very valuable indeed though costly.

Experiences with trunk piston type of oil engine have been related in detail not because it is expected that many of them will be built in the future, but because so many questions which will come up with more modern types have already been answered by experience with them. Slow speed engines for direct drive versus high-speed engines with gear drive will for instance come up again and again in the future. The direct drive installations in the *Rheinland* class (6 cylinder 28.35-inch $\times$ 51.18-inch) have been quite successful. Fuel consumption is practically the same as with the ex-submarine engines. The average for the whole trip including auxiliaries amounts to .308 pounds of oil per indicated horsepower. The air consumption for maneuvering is considerably larger than for the high-speed engines and an auxiliary air compressor of liberal size is absolutely necessary. Main engine suction air piping leading above deck throttles the amount of air sucked into the working cylinders and the engines suffer from insufficient air. It would appear that the solution would be to permit air intake direct out of the engine room without any special suction piping, but this would lead to uncomfortably low temperatures in winter. The main reason against it, however, is the excessive humidity in the tropics which would cause everything to rust in the engine room if combustion air be permitted to

circulate through the engine room on the way to the cylinders, and the atmosphere is often so salty that everything would be covered with a layer of salt. It is therefore wiser to foresee both these unpleasant consequences and use liberally dimensioned ducts to bring combustion air to the engines, care being taken to avoid resonance in this piping, since undue resistance is often caused not by the cross-section of the duct, but by the appearance of resonance with vibrating air column. A special electrically driven blower could be installed not to increase the output of the engines, but merely to avoid low pressures in the suction piping and obtain cleaner combustion air. This would result not only in a slightly increased output but would also lower the fuel consumption per horsepower.

Exhaust piping cooled by sea water has been causing considerable trouble and the double-walled cast iron pipes have cracked frequently due to difference in temperature between the inner and outer walls. Cast iron expansion pieces proved insufficiently elastic, and the jacket chambers are not everywhere accessible enough. This led to the gradual adoption of sheet steel, uncooled but effectively insulated exhaust pipes, where elasticity can be obtained much more easily.

On the engines of the *Rheinland* and *Ermland* the bottom parts of the working cylinders are not separated from the interior of the crank case by means of so-called lanterns, so that lubricating oil consumption is relatively high although considerably lower than for the trunk type. For these 4300 I.H.P. installations it amounts on an average to 41 gallons (308 pounds) per day at sea including all auxiliaries.

Working pistons on the *Rheinland* and *Spreewald* are cooled by salt water, without any difficulties arising therefrom. The erosions on the piston rods of the *Rheinland* and *Ermland* at the inlet and outlet of cooling water are characteristic. Here most likely strong corrosive eddies occur due to a sharp change in direction, or the erosion may result from hydraulic hammer disintegrating the material. At any rate these places had to be often repaired by adding metal with a welding torch.

Foundations under the big engines had to be reenforced later, as foundation bolts and rivets on the tank top would get loose. The engine air compressor driven by a crank on the main crankshaft had a common foundation with the engine itself. Relative motion in the foundation due to running of the engine created such stresses in the structure that the compressor foundation was completely separated. No further trouble was experienced and it was decided to leave the break as it was, since it appeared wiser to permit existing but harmless relative motion instead of preventing it by heavy reenforcements.

The B. & W. engines built by the A. E. G., as the propelling plant of *Spreewald* and *Odenwald*, use from 14½-17½ gallons (110 to 130 pounds) of lubricating oil per day at sea for a total of 3300 indicated horsepower. These engines also gave good service with little repair work necessary. The through anchor bolts for the transmittal of forces from cylinder head unto the foundation, as they are used in Burmeister and Wain type of engines, have to be tightened from time to time to take up the permanent stretching due to engine operation.

In summing up the experiences with motorships it must be said that none of the different types of engines used have shown outstanding superiority. Fuel consumption is practically the same. Even the repair costs show no appreciable difference. Lubricating oil consumption for engines with the crank case completely separated from the working cylinders is decidedly better, hence for new construction no trunk piston type engines will be built and separation of crank case will be insisted on. The direct drive requires

considerable height of engine room but has better fuel consumption. On the other hand the propeller speed can be chosen quite freely when gear drive is employed, with an additional advantage of connecting two engines to one propeller shaft, which has already been done on several occasions. Maneuvering even with a non-elastic connection of engines to gear drive offers no practical difficulties.

The heat of the exhaust gases has been utilized but very little to date, although the results of existing installations thermodynamically are exceedingly encouraging. The best way to use the heat in the exhaust gases is naturally for heating either in air or steam systems or for heating water where such is necessary. But auxiliaries are also driven by steam produced in exhaust gas boilers quite successfully. There is very little choice of auxiliaries suitable for this purpose since most of them must operate also in port when the main propelling engines are idle, or at any rate must be started prior to starting up the main engines. Hence auxiliary oil firing must be added to the boiler which is not complicated in itself but increases the cost somewhat. In such cases the more expensive oil engine fuel is usually used in order to avoid extra tanks and piping, and a condensing apparatus is required. Hence, it is quite natural that even in modern motorships in order to simplify the installation the heat in the exhaust gases is seldom used. The use of steam in a steam turbine connected by gears to the main shaft appears to be more worth while since no oil firing would be required and boilers could be built for exhaust gases only and used in place of exhaust mufflers. And even the heat in the circulating water could be profitably used in the low pressure stage of the turbine.

The use of an oil engine propelling plant has resulted in a complete change of the auxiliaries. Since electricity is comparatively cheaply produced by oil engine driven generator sets, it is natural that these are employed. The horsepower requirement is determined by the necessary maximum with a suitable reserve in addition. This maximum requirement becomes necessary on entering port when the large auxiliary air compressor is called upon to furnish maneuvering air.

Although 110-volt circuits are usual in the merchant marine practice, 220-volt system was chosen for the driving of auxiliaries on the motorships. Since rather heavy cables have to be used the advantage of the smaller cross-section of copper wire overbalances the increase in cost of heavier insulation necessary. Practical experience has shown no difficulties whatever. In ships of the "Land-Class", it was hoped to get along with two oil engine generator sets of 110 K. W. capacity, but a third one had to be added later. Very good results have been obtained with auxiliary generator sets, so good, in fact, that oil engine driven generators are being installed now even on large steam turbine driven passenger liners for the generation of the necessary electrical energy.

Small cylinder dimensions of the auxiliary oil engine do not absolutely require piston cooling. Exhaust valves are also uncooled. This service, requiring engines of comparatively small output, gave a chance to try out the airless injection type of engine under service conditions, which are after all still quite different from the test stand. Even the fuel used is quite different and varies with every bunkering station. The prime requirement, of course, of these auxiliary oil engines function is reliability, since the safety of the complete installation is dependent upon them. Regulation must be as good as for turbo-dynamos especially when the generators, as it often happens, must work in parallel. In order to forestall overloading of one engine when the other is disabled for some reason or another a safety switching is used, which automatically cuts out less important circuits.

Three-cylinder auxiliary oil engines are preferred to the two-cylinder type, although even these gave no serious trouble. It is better that even on these small engines the pistons be made in two parts and eye bolt holes should be left out altogether, as they are in time the starting point of cracks. The attached air compressors should not be made too small, so that excessive heating and consequent wear of piston rings and cylinder barrel will not occur.

The generator is direct coupled to the oil engine. Contrary to the experience with the turbo-generators no commutation troubles were experienced with oil engine driven generator sets. The heavy oil vapor resulting from tropical temperatures leads to dirt accumulations in the fields. All electrical machinery suffers from that very much. The layer of oil finally dissolves the insulation and jeopardizes the continuity of operation. High rotative speeds and over-rating of electrical machinery require intensive cooling of the field and when the atmosphere around is oil laden the above troubles result. Hence suitable alterations were made to take cooling air not out of the room but by means of special ducts lead it from above the decks; the only precaution necessary is to protect the intake so that neither sea nor tropical rains can enter the system.

The auxiliary air compressor for the main propelling plant on some of the installations is connected to the auxiliary oil engine by means of a clutch, although in more modern installations a motor-driven compressor is preferred. The auxiliary air compressor itself should be liberally dimensioned. Inefficient cooling has been the cause of numerous shutdowns.

Lubrication of engines is of the forced feed type, although efforts are being made to substitute a reliable drop-feed type instead, in the hope of reducing the lubrication oil consumption considerably. Large quantities of lubricating oil are thrown (particularly by the short-stroke ex-submarine motors) into the cylinder bore and not scraped off sufficiently, thus causing the excessive lubricating oil consumption. But the drop-feed gravity system can not carry away enough heat from the bearings, it is feared, hence no favorable decision to try it has been reached as yet. Lubrication of the wrist pin of the main engine air compressor also offers special difficulties, since it is easily lubricated through the hollow crankpin and connecting rod with the force feed system, but can not be done so with drop feed. However, even here a solution will be found, no doubt, probably similar to that used on small direct driven air compressors where next to the connecting rod a pipe goes up into the interior of the piston and is mounted on a bracket fastened to the crank case. Saving in lubricating oil consumption will always be an incentive to make such a test, since oil prices are very high and oil losses excessive. The question naturally arises, what happens to the lubricating oil? On engines without a lantern between cylinders and crank cases it is easy to assert that a large part of lubricating oil is burned up in the working cylinders of the engine, whereas in totally isolated crank cases the lubricating oil is so much thrown around that it atomizes and vaporizes and a large part of it disappears as oil vapor into the atmosphere. The higher the temperature the finer the atomization, hence, a better ventilation of crank case reducing the interior temperature would serve also to reduce materially lubricating oil consumption.

The Neidig type of gear pumps serve almost exclusively as lubricating oil pumps and with pronounced success. The noise of these pumps was originally so loud that it was found objectionable. This, however, was improved especially by the use of rawhide gears. Cooling and cleaning of lubricating oil are problems the importance of which is being more and more recognized. Lubricating oil consumption is a direct function of tempera-

ture, so that in the tropics for instance with warm sea water the oil consumption rises to double the amount used in our more temperate climate. Cooling of oil is made more difficult by too close spacing of cooling pipes which are easily covered by a tough, insulating layer of oil which can not be removed neither by a steam jet nor mechanically.

Oil is usually circulated outside of the pipes to take advantage of larger cooling surface. It is more important, however, to keep the surfaces clean and it is simpler to make cleaners when the oil passes through the tubes and the water outside of them. In order to keep the oil clean all the time, it is important first of all to find means for preventing it from getting dirty. Coking of oil in the piston cavities and on cylinder walls is largely responsible for that, and it is therefore necessary that oil be mechanically circulated in the piston, to prevent dead pockets where cooling is ineffective. Oil in contact with excessively hot piston walls cokes, with piston cracking as the eventual result.

The construction of the engine must be such as to prevent throwing up of oil from the crankpits onto the cylinder walls, which probably means that a lantern between the cylinders and crankcase should be provided. If that is not possible, suitable splash guards to catch the larger part of the spraying oil should be installed. Occasional blowing past the piston rings is another type of pollution of oil, but this can hardly be prevented. Care must be taken furthermore that no circulating water gets into the lubricating oil. Such a requirement is very difficult of accomplishment in service with knuckle joint or telescopic pipes. It has already been mentioned as remarkable how large a quantity of oil gets into the circulating water despite all the scraping devices and stuffing boxes used on the telescopic pipes and conversely how much water gets into the lubricating oil. The dirty oil can be cleaned by storing in large settling tanks or better in centrifugal separators of various types, the best known of which are Westfalia, Alpha-Delaval and Sharples. These are all similar in principle and all give equally good results. Fresh water is much easier separated from oil than salt water. Heating of oil up to 175 degrees F. makes separation easier and in some cases only then is it at all possible.

That only the best quality of lubricating oil should be used for the oil engines is quite natural, and compared with turbine installations it must be changed oftener. Normally oil hardly ever stays over one year on board and about four months in actual service.

Electric driven centrifugal pumps are used almost exclusively for circulating water pumps. They do not offer anything new and their power requirements are low, as an efficiency of 65 per cent is now attainable. The three-crank pump aggregate consisting of sanitary, ballast and bilge pumps had at the beginning the objection that it used trunk plungers and the wrist pin lubrication would give considerable trouble. The new type of pump is built much more substantially and has proven quite satisfactory in service.

Nowadays all larger ships are equipped with ice machines which on motorships are motor driven. They use but very little current and such drive is much cheaper than steam. Besides the compressors are of the high-speed type so that their overall dimensions are very small. Instead of motor drive a small Benz oil engine with airless injection direct coupled has also been used, so that a completely independent refrigerating system is possible. These modern refrigerating installations have been recently described by Director Goos.*

* See "Werft-Reederei-Hafen," Feb. 22, 1926, p. 87.

[Discussion of rudder and steering gear, anchor capstans, electric winches, their controllers and cables follows. This has not been translated, as of limited interest.]

In conclusion the air whistles which had to be used in place of the steam whistles should be mentioned. Their tone is not as clear and sharp as that of the steam whistles, and the air consumption not economical enough. Since they are used when entering port when the air consumption is already high, it is quite natural that a better solution of the problem is being looked for. The well-known Tyfon reduced the air consumption to one-half and is being used especially for fog signals, which must be periodically repeated. A new electric-driven whistle, which exceeds in tone qualities all type of air whistles used so far, is installed on the more modern motorships of the Hamburg-America Line. This is, however, not yet beyond the experimental stage. A serious objection of all the air whistles as compared to those operated by steam is that steam is visible and air is not.—"Werft-Reederei-Hafen," April 7, 1926.

AMERICA'S LARGEST FOUR-STROKE-CYCLE DIESEL ENDS TESTS.

The long gruelling test on the first of the 2700-horsepower four-stroke-cycle single-acting Diesels built by the McIntosh & Seymour Corp. for the Shipping Board was finished on March 29. The second engine has been completed and is undergoing a similar test. Much interest centers in these two units since they are the largest four-stroke-cycle engines built in this country.

The engines follow the general lines of the corporation's standard crosshead type engines. The cylinders are supported on vertical cast-iron frames which in turn rest upon the bedplate. Between the crankcase and the cylinder is interposed a box casting which seals the crankcase against contamination by fuel-oil drippings or water leakages. The camshaft running along the frame at a level with the cylinder bases is driven from the crankshaft by a spur-gear train. The valves are operated by a system of rockers and push rods. To reverse, these push rods are withdrawn from contact with the cams by the action of an eccentric operated shaft. The reverse control gear then shifts the cam-shaft longitudinally until the proper cams are under their respective valve-rod rollers.

Air is used on all the cylinders for starting, and as soon as the engine is in motion under the influence of the starting air, all the fuel valves are thrown into action.

The piston is of the crosshead type, and the cooling water is conducted into it through telescopic tubes. The tube joints are placed above the crankcase top, and any leakage is conducted away through drip lines.

The main engine has no built-in air compressor, and a 400-horsepower auxiliary 4-cylinder unit is used for this purpose as well as to supply the electricity needs of the ship. This unit follows the standard boxframe designs used on stationary engines.

In addition there is a 108-horsepower unit direct-connected to a 75-kilowatt generator and provided with a 310-cubic foot compressor, so that in the event of a failure of the compressor engine, air can be obtained from this small unit.

The whole engine, together with its auxiliary, was run at full load for 30 days, after which a 10 per cent overload was carried for 6 hours, immediately followed by a 4-hour run at 3,100 horsepower. The power output

was measured by a calibrated water brake, and as a consequence there can be no question as to the accuracy of the load and fuel consumption.

The combined fuel consumption of the main and auxiliary engines worked out as being 0.409 pound per brake horsepower-hour delivered at the shaft of the 2700-horsepower unit. This is exceedingly low inasmuch as the frictional losses of the auxiliary should be considerably greater than the friction developed in the air-compressor cylinders if they had been direct connected to the main engine. In addition, the fuel used at least during part of the test was very heavy and had a sulphur content of 4.8 per cent. So decided was the sulphur that the exhaust had a most unpleasant odor.

Combustion necessarily, in view of the fuel economy, had to be good, and the clear exhaust indicated that nothing was left to be desired.

The principal dimensions of the units are given in the tabulation.

	Main Engine	Independent Air Comp. Unit	Aux. Comp. Gen. Set
Brake horsepower	2,700	400	108
Speed, R. P. M.	95	200	275
Bore of cylinder, inches	32	217	13⅝
Stroke, inches	60	24	
Weight, pounds	800,000		
Size of generators		75	75
Displacement of air compressors, cubic feet per minute		936	310
Floor space required, feet	40×11½		
Diameter of crankshaft, inches	21		
Length of crankshaft, feet	40		
Total height (approximate) feet	32		
Total length, feet	40		
Total width, feet	11½		
Diameter of exhaust valve, inches	10¾		
Diameter of inlet valve, inches	10¾		
Weight of largest casting, pounds	65,000		
Cooling water per hour, gallon	11,000		
Fuel consumption, main engine and independent compressor unit combined, per brake horsepower-hour, pounds	0.409		

It is expected that the two units will be installed on the *West Honaker* and the *West Cusseta*.

Two double-acting engines ordered by the Shipping Board are now in the process of construction and will probably be ready for test during the summer.—"Power," Apr. 27, 1926.

HIGH-PRESSURE STEAM INSTALLATIONS.

Should the tendency towards the use of higher steam pressures, as exemplified by the *King George V.* and to a lesser extent by the projected Canadian Pacific liners, develop into an established practice, it would undoubtedly lead to many fundamental changes in the whole technique of the design and construction of machinery for use on board ship. That this would be so can be gauged from the paper which Mr. H. E. Yarrow read at the recent meetings of the Institution of Naval Architects in which he described the boilers for

the *King George V.* These, it will be recalled, while embodying the recognized features of the Yarrow boiler, as far as the relative arrangement of drums and tubes was concerned, differ very greatly from what by comparison might be termed the low-pressure type, having a working pressure of 220 pounds per square inch, in that an attempt has been made to eliminate the riveted end plates from the drums. It is not suggested that the method of forming the drums adopted in these boilers will become established practice for all high-pressure work, but it will be realized that as far as possible, the number of riveted joints must be reduced to a minimum, and this is bound to have some effect on the design of the boilers. It will be of interest to see if the type of drum used on the *King George V.* is adopted on the Yarrow boilers for the Canadian Pacific boats, where the pressure is not 550 pounds per square inch but 350 pounds per square inch. Again, it is difficult to see how at high pressures the ordinary large drum return-tube type of marine boiler can be employed unless the boilers are made of excessive weight. The well-known formula for finding the thickness of the boiler shell $t = pd/2f$ shows that the thickness varies directly as the pressure, so that if the pressure is doubled, the thickness of the plates forming the drum must be doubled, other factors remaining the same. This means that at 500 pounds per square inch the thickness of the boiler shell plating would be in the region of 2 inches, which is, of course, unthinkable. Thus, it might almost be stated categorically that the use of high steam pressure involves the use of the water-tube boiler. On land, where in many large electric generating stations high-pressure steam plant is being increasingly adopted, the water-tube boiler is invariably employed, and while conditions at sea are admittedly different from those on land, the success of the water-tube boiler on land should give confidence to marine engineers who adopt it on board ship.

The use of high-pressure steam will not, however, react only on the design of the boilers. It will—although, perhaps, to a lesser extent—affect the design of the main engines, the condensers, and even the auxiliaries. For the main engines—particularly if the steam coming from the boilers is highly superheated—we may expect to have a small high-pressure turbine of the impulse type in which the steam will expand down to normal temperature and pressure before impinging on the blades, thus reducing the number of blade wheels subjected to the erosive effects of the superheated steam to a minimum. This extra high-pressure turbine, owing to its relative high speed of rotation, would probably be arranged in a separate casing, and be connected to the main speed reduction gearing by means of a small pinion, thus giving the requisite ratio of speed reduction. Subsequent to its expansion in this turbine, the steam would pass to a second casing at a pressure of, say, 250 pounds per square inch and, unless reheating is employed, at about saturation temperature. In this way the whole of the effects of the high-pressure steam would be confined to what we have termed the extra high-pressure turbine. It has been assumed that the turbine would be adopted in conjunction with high-pressure boilers, but there is no reason why the reciprocator should not be utilized, particularly if only a small amount of superheating is employed. The change in the specific volume of the steam at high pressures is not very great and the uniflow and drop valve type of reciprocating engine, with their small clearance volumes, could deal almost as easily with steam at 400 pounds per square inch pressure as at 200 pounds per square inch, and while the distortion effects due to a high degree of superheat in the steam might be troublesome to deal with, it must not be forgotten that in Diesel engine practice we have become familiar to pressures just as high as, and to temperatures far in excess of, those suggested by the most enthusiastic advocate of the use of high-pressure high-superheat steam.

The conditions in the condenser of a high-pressure steam installation are the same as those in one of normal design, but in view of the conditions in the boiler and the necessity for keeping the condensate absolutely free from contamination by sea water, it is apparent that very great care must be taken in the design of the condensers to reduce the risk of tube failure and of leakage at the glands to a minimum. The use of two condensers, one of which can be cut out for repairs when such leakage does occur, does not appear to yield a satisfactory solution of the problem, particularly on vessels of large power, but it is certain that delicate salinometers must be continuously in use, and at the least suggestion of excess of salt in the condensate, means must be taken to prevent the accession of salt into the system and to stop that salt from reaching the boiler. This is a problem of vital importance, and it is not too much to say that upon its successful solution rests the future development of the use of high-pressure steam installations on board ship. Mention must be made, in conclusion, of the possible changes which may occur in the auxiliaries when high-pressure steam is employed for the main engines. In order to avoid having to design the small auxiliary engines to resist the effects of high pressure and high temperature, it might be suggested that the steam should be delivered through a reducing valve at a more normal pressure. This, however, leads to further superheating and in any case is a rather uneconomical procedure. A better alternative would be to by-pass the auxiliary steam through the extra high-pressure turbine, tapping the receiver between this casing and the subsequent one for the steam required for auxiliary purposes, or since this would not avoid the use of a reducing valve when the main engines are at rest, it would be still better to employ a reducing pressure turbine for driving the auxiliary generators, the exhaust from which could be utilized for the steam-driven auxiliaries or else by-passed to the main engines, or utilized for feed-heating purposes, the bulk of the auxiliaries being electrically driven. In this way the maximum economy will be obtained from that large proportion of steam represented by the consumption of the auxiliaries.—" Shipbuilding and Shipping Record," June 17, 1926.

PROPOSED METHOD FOR THE AUTOMATIC SUPERHEATING OF STEAM.

It would appear at first sight as an impossible thing that steam could be raised in temperature except either by the application of heat at some temperature higher still, or by the use of some mechanical agency such as a compressor. That steam may be superheated to a certain extent by throttling is, of course, well known; but that is another matter. After being throttled, steam is in a superheated condition not because its temperature has been raised, but because the saturation temperature corresponding to its new and lower pressure is less. Its actual temperature is indeed somewhat less than before throttling, owing to the "Joule-Thomson effect." To get a final temperature higher than the initial temperature without the use of external heat would clearly be possible by using a part of the steam to drive a compressor which would compress the remainder of the steam and thus raise its temperature. Such an arrangement would be automatic in the sense that it would operate without any supply of heat or power from outside, but there is no point of view from which it would appear very attractive. The necessity of having an engine and compressor, even without considering the unavoidable losses to which they would give rise, would be sufficient to condemn the scheme from any practical standpoint. It has, however, been suggested, originally, we believe, by Professor Watkinson

and more recently by Mr. R. H. Parsons, that there is, at least theoretically, a way by which steam may be automatically raised in temperature without the use of any running machinery whatever. The principle is the reverse of that employed in an ordinary refrigerating cycle. In the latter the working fluid is temporarily heated by compression. Heat can be abstracted from it when in this condition, so that when restored to its original pressure it is cooler than before.

The converse of this process as applied to the heating of steam would be temporarily and artificially to cool the steam by expansion in order that heat might be able to flow into it. The acquisition of this heat would result in a higher temperature when the original conditions of pressure were restored. Since, by hypothesis, the steam is below its original temperature at the time of receiving heat, it could obviously absorb its additional heat from some of the original unexpanded steam. Thus, at the expense of some of the original steam, we could deliver the remainder at a higher temperature, and the process would go on automatically. It could, moreover, be repeated a number of times, raising that part of the steam which was not used up in the process to a temperature far greater than that which any of the steam originally possessed.

To carry the idea into effect, steam might be caused to flow, under a very slight difference of pressure, through a tube of Venturi form. Adiabatic expansion would take place in the converging entrance portion of the tube, resulting in the steam having a lower pressure and temperature when passing the throat. The pressure drop at the throat has, of course, been taken advantage of in the design of steam meters. The temperature drop, however, should make it possible to impart heat to the steam at that point by jacketing the throat with steam of the original temperature. In the diverging part of the Venturi tube, under ideal conditions, adiabatic compression would take place, the kinetic energy produced during the previous adiabatic expansion effecting an adiabatic compression of the steam and so restoring its pressure. If it should be found practicable to raise the temperature of steam in the way suggested, the idea may find useful application in connection with the reheating of steam between the high and low-pressure cylinders of a steam turbine. There would be no need to return the steam to the re-superheaters in the boiler-room, as the required temperature could be obtained at the turbine itself, a certain amount of steam, naturally, being used up in the process.

There is, of course, no theoretical thermodynamic gain by heating steam in the way proposed. One can always be certain that any action which takes place by itself in Nature involves a loss of available energy. Heat will not up-grade itself on the whole, any more than water will run uphill. The fall of some of the water, however, may be used to pump the rest to a higher level, although the final result is inevitably that there is less energy available for work than there was before. It may be, however, that 1 pound of water at the height of 100 feet is more to be desired than 2 pounds of water at a height of, say 60 feet, and under such circumstances the loss of available energy is justified. If one is content to exchange a quantity of steam at a low temperature for a less quantity at a high temperature, and to ignore the increase in entropy which the conversion involves, then a self-heating process may have its advantages. Whether it is possible in practice to get any considerable amount of heat into steam during the brief period of passage through the throat of a Venturi tube is a matter for experiment. The velocity of the steam will be of the order of 1500 feet per second, so that the time is short for heating, with any reasonable length of throat. By proportioning the tube with a very small hydraulic mean depth circumstances would be more favorable to heat

transmission, but, on the other hand, frictional losses would be increased and the efficiency of the tube as a whole would probably be greatly decreased. To carry out the principle of self-heating of steam in actual practice might therefore be difficult, but the idea is certainly an interesting one.—"The Engineer," May 21, 1926.

LIMITING FACTORS IN REDUCING EXCESS AIR IN BOILER FURNACES.

By E. G. Bailey,* Cleveland, Ohio.

Everyone interested in the combustion of fuel in boiler furnaces realizes the importance of maintaining a low percentage of excess air. If the air for combustion is reduced to a point too close to the theoretical requirements for a given fuel, however, several other factors must be considered or else poorer efficiency and a higher operating cost will be encountered than if more excess air had been permitted.

The three factors most closely related to excess air are furnace temperature, unburned fuel, and heat loss in the chimney gases. There is also the effect upon superheated-steam temperature and the power required to handle the air to the furnaces and the products of combustion from the boilers. With given equipment and fan capacity the reduction in excess air has permitted operating the boilers at higher capacities.

The first three factors are the ones of greatest interest, and they are the ones which will be discussed particularly in this paper. The interrelationship between these factors is fundamental, and a study of their relative importance and consequence is of wide interest as it applies to all types of fuels, furnaces, and operation.

The reduction in excess air means higher furnace temperature—which in itself is a good point for combustion efficiency—but consideration must be given to the cost of furnace maintenance such as refractories, stoker parts, and burners, as well as to the formation of clinkers—all of which tend to increase the operating cost. A reduction in excess air beyond the point of the theoretical requirements will of course result in incomplete combustion of the fuel, and due to imperfect mixtures this costly result really begins to show itself long before the air has been reduced to what is known as the theoretical amount necessary for complete combustion.

By unburned fuel reference is made not only to CO and other combustible gases but also to carbon and smoke in the flue gases and combustible in the ashpit, all of which are more or less closely associated with the percentage of excess air.

Before the days of the combustion engineer and before the CO_2 recorder and other similar equipment were available, every fireman used his more or less good judgment in striving for the best percentage of excess air. He did not talk of CO_2 or excess air, but he used the expressions, "a hot fire," "keep the holes out of the fire," and "carry a fuel bed of the right thickness," giving little or no attention to the question of smoke.

The impression seems to be rather general that in olden days the biggest trouble was with excess air, but excess air prevailed in the plants that had plenty of draft, while a deficiency of air and unburned gases prevailed in plants that had limited stack or draft facilities.

* President, Bailey Meter Company. Mem. A.S.M.E.

Presented at a meeting of the Metropolitan Section of the A.S.M.E., New York, April 8, 1926.

During the past ten years the rapid development of mechanical stokers, pulverized coal, improved oil- and gas-burning equipment, and the study of the combustion problem have brought out very many interesting facts. Even in plants where combustion engineers were not continuously studying the problem, the operator has naturally tended to reach the most economical percentage of excess air through experience and observation, with smoke and refractory trouble on the one hand, and low steaming capacity and poor-looking fires as the result of excess air on the other.

During this period of time the author has been closely in touch with the situation, as combustion tests have been made under his direction in connection with the installation of about 4000 boiler meters recording the steam output, the air flow, and, in most cases, the flue-gas temperatures or other factors closely related to boiler and furnace operation. When the data thus obtained are classified and averaged for different fuels and different types of fuel-burning equipment, they have a very significant value in that they represent definite tendencies and relative characteristics, the same as does any mass of statistics compiled, whether relating to weather or the mortality of human life.

The data presented in this paper represent more than 75,000 complete flue-gas analyses taken across the last pass of the boiler, with due care being exercised to avoid errors due to stratification or air leakage. The Orsat apparatus was used for determining the CO_2, oxygen, and CO of each sample.

DATA ON COMBUSTION TESTS.

Figure 1 represents a summary of 3,767 combustion tests classified as to fuel—coal, oil, and gas—coal being subdivided as to method of firing into hand fired, overfeed stokers, natural- and forced-draft chain-gate stokers, forced-draft underfeed stokers, and pulverized coal; while oil has been divided between steam-atomized and mechanical-atomized burners. These are arranged in order of the diminishing percentage of excess air from the

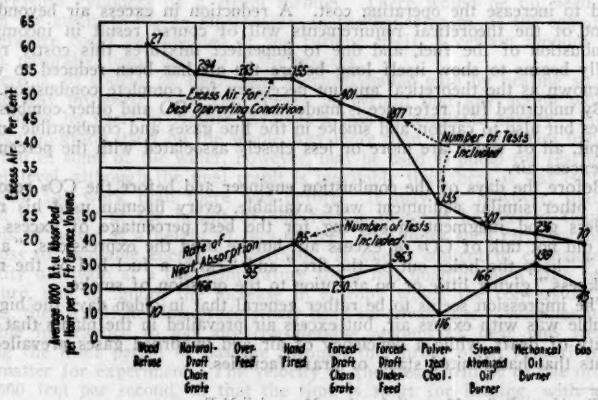


FIG. 1.—AVERAGE EXCESS AIR AT BEST OPERATING CONDITIONS WITH DIFFERENT TYPES OF FUEL-BURNING EQUIPMENT.

maximum of 61 per cent attributed to wood refuse, down through 45 per cent for underfeed stokers, 28 per cent for pulverized coal, and 19 per cent for gas-fired boilers.

These results were obtained over a period of ten years and represent all types of boiler and furnace equipment collectively. However, they should not be taken as representing the best that can be obtained in each individual class, but merely as averages that were found when operating under normal, every-day conditions, with a combustion engineer present analyzing gases, studying conditions, and establishing standards at which the equipment was subsequently operated as being most efficient.

The lower curve shows the rate of heat absorption by the boiler and superheater per cubic foot of furnace volume, this being merely indicative of the different types and methods of burning. It is to be noted that the number of tests in the lower line is less than that on the upper, which is due to the fact that complete data as to furnace volume were not available in all cases.

Figure 2 represents the same data, but with reference to the limitations which prevented the further reduction of excess air.

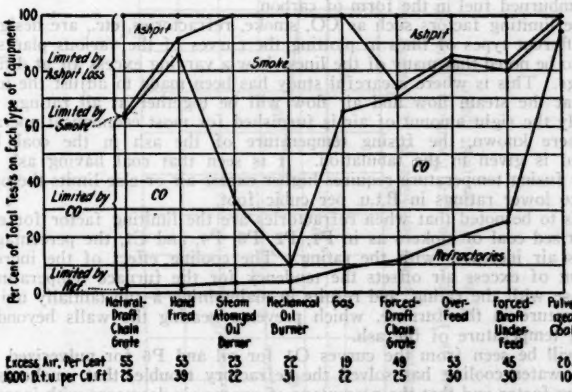


FIG. 2.—RELATIVE FREQUENCY OF OCCURRENCE OF THE VARIOUS LIMITING FACTORS AT NORMAL RATINGS ON DIFFERENT TYPES OF FUEL-BURNING EQUIPMENT.

It is to be noted that CO predominates as the limiting factor in all cases where coal is burned in fuel beds and also in the gas-fired boilers, while smoke is the guide in determining the minimum excess air for oil.

Ashpit loss comes in as a fairly important factor as regards the mechanical stoker, and in the more modern types of stokers the refractories obtain more attention than in the earlier types where higher excess air prevailed.

Pulverized coal has been limited in excess-air reduction almost entirely by refractories. In other words, the furnace-temperature control is a matter of prime importance due to the large area of walls and the fact that the ash is carried in suspension and deposits on the walls, thus diminishing the resistance of the latter to slagging and erosion.

RELATION BETWEEN PERCENTAGE OF EXCESS AIR AND RATE OF COMBUSTION.

It would hardly be proper to give only the averages of the different groups and types of equipment without showing what results have been accomplished in some of the individual installations of the later and more modern types. Therefore, in Figure 3 are shown plots of several of the more modern plants giving the relationship between percentage of excess air and the rate of combustion. For this purpose the number of B.t.u. absorbed by boiler and superheater per cubic foot of furnace volume per hour has been adopted as a basis for comparison. It is realized that this is not the figure generally given, because usually the number of B.t.u. developed per cubic foot is determined from the coal fired. However, in work of this kind it is not always possible to determine the weight of coal burned, but it is a comparatively simple matter to determine the rate of B.t.u. output as shown by the flow meter, and this is always available in connection with the work represented herein.

From another standpoint the number of B.t.u. absorbed is better than the number put into the furnace, because all of the heat is not necessarily developed into usable form, as in the case of high loss due to unburned gas and unburned fuel in the form of carbon.

The limiting factors such as CO, smoke, refractories, etc., are designated by different types of lines in plotting the curves of the various plants, and it is to be noted that many of the lines show a varying excess air at different ratings. This is where a careful study has been made to adjust the meters so that the steam flow and air flow will be together at all ratings when exactly the right amount of air is furnished for most economical operation.

Where known, the fusing temperature of the ash in the coal being burned is given in the tabulation. It is seen that coal having ash of a lower fusing temperature requires higher excess air or else limits the operation to lower ratings in B.t.u. per cubic foot.

It is to be noted that when refractories are the limiting factor for either pulverized coal or stokers as in P1, P2, P3, P4, and C1, the percentage of excess air increases with the rating. The cooling effect of the increased amount of excess air offsets the tendency for the furnace temperature to increase with the rating, and results in maintaining a substantially uniform temperature in the furnace, which prevents heating the walls beyond the fusing temperature of the ash.

It will be seen from the curves O1 for oil and P6 for pulverized coal, where water cooling has solved the refractory trouble, that smoke is the limiting factor and that the percentage of excess air decreases with increased rating; or, to state this inversely, when the temperature of the furnace is decreased, the percentage of excess air must be increased to offset the lowering of temperature in order to get anything like complete combustion.

The underfeed stokers are plotted with CO as the limiting factor in most cases, although it may be that unburned coal and refractories should be given a little more consideration than they have been.

Curves U5 and O2 are taken from the same boiler and furnace adapted to burn coal or oil. In this as in some of the other smaller furnaces of older design and a high rate of B.t.u. per cubic foot, the refractory trouble is minimized because of the radiant-heat-absorbing capacity of the front row of boiler tubes.

Values for oil are much higher with small furnaces and mechanical burners, even reaching 85,000 B.t.u. absorbed per cubic foot of furnace volume in some cases. In most cases smoke is given as the limiting factor in reducing excess air in oil-fired furnaces, although sometimes the life of

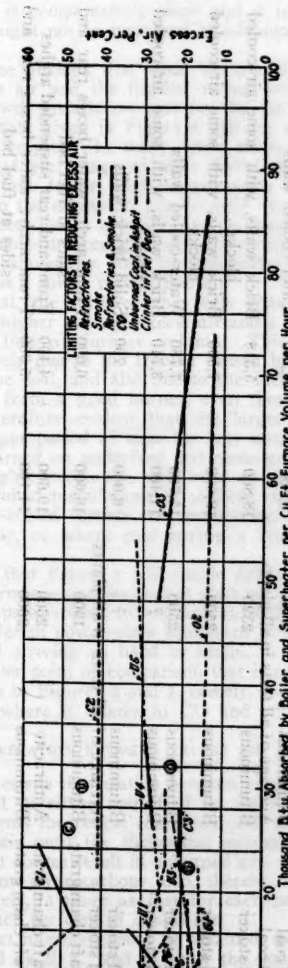

Excess Air, Per Cent
Thousand B.t.u. Absorbed by Boiler and Superheater per Cu. Ft. Furnace Volume per Hour
LIMITING FACTORS IN REDUCING EXCESS AIR
Refractories.
Smoke
Refractories & Smoke
CO
Unburned Coal in Ashpit
Clinker in Fuel Bed

FIG. 3.—RELATION BETWEEN PERCENTAGE OF EXCESS AIR AND RATE OF COMBUSTION IN A NUMBER OF MODERN PLANTS.

[See table on next page.]

Plant	Fuel-Burning Equipment	Kind of fuel	Fusing Temperature of Ash, Deg. Fahr.	Area of Boiler Surface, Sq. Ft.	Furnace Volume, Cu. Ft.	Furnace Details
P1	Pulverized coal	Bituminous	2000	30,000	26,000	Hollow walls; water screen
P2	Pulverized coal	Bituminous	2000	18,000	12,500	Hollow walls; water screen bottom and back wall
P3	Pulverized coal	Bituminous	2200	5,000	4,200	Solid walls, front, sides and floor; air-cooled back wall
P4	Pulverized coal	Bituminous	2050	14,000	11,100	Hollow walls; water screen
P5	Pulverized coal	Bituminous	2350	17,700	12,000	Hollow side and front walls; water screen in bottom and superheater in back wall
P6	Pulverized coal	Bituminous	2000	5,600	3,150	Water-cooled walls
(P)	Pulverized coal	Average of all tests in all plants				
U1	Underfeed stoker	Bituminous	2320	18,800	5,318	Brick walls, with some air-cooled blocks
U2	Underfeed stoker	Bituminous	2500	19,600	7,500	Brick walls, with some air-cooled blocks
U3	Underfeed stoker	Bituminous	2500	15,700	7,780	Water-cooled walls
U4	Underfeed stoker	Bituminous	?	11,400	3,710	Brick walls, with some air-cooled blocks
U5	Underfeed stoker	Bituminous	?	6,000	700	Solid brick walls
(U)	Underfeed stoker	Average of all tests in all plants				
C1	Forced-draft chain grate	Bituminous	1900	16,600	6,000	Front arch; water boxes rear and sides at fuel bed
C2	Forced-draft chain grate	Anthracite	2600	19,600	4,100	Front and rear suspended arches
C3	Forced-draft chain grate	Bituminous	2000	14,100	5,000	Front arch; water boxes rear and sides at fuel bed
(C)	Forced-draft chain grate	Average of all tests in all plants				
G1	Multiple-jet burners	Natural gas	....	7,000	1,860	Solid brick walls
(G)	All kinds of gas burners	Gas of various kinds, average of all tests in all plants				
O1	Mechanical oil burners	Crude oil	....	14,920	13,000	Hollow walls and some water-cooling tubes in floor
O2	Mechanical oil burners	Oil	....	6,000	700	Brick walls; no water cooling
O3	Mechanical oil burners	Oil	....	5,200	725	Brick walls; no water cooling
(O)	Mechanical oil burners	Average of all tests in all plants				

the refractories is comparatively short and it is a question as to whether the excess air might not be increased to advantage from the standpoint of total cost.

It is of course obvious that what we are all striving to attain is the minimum excess air and the highest rating for a given furnace volume, which means lower installation cost for the furnace equipment. It would seem from the data given in Figure 2 that oil as a fuel holds the leading position in this respect. In many ways oil is ideal in that it does not contain any great percentage of ash; therefore the fluxing of the walls is not an important factor, high temperatures can be obtained, and with good atomization the excess air can be held very low.

In many respects gas is a better fuel to handle than oil, but due to its bulk and the trouble of thorough mixing, it usually requires a larger furnace volume, and as shown by both the individual and average results, it is usually burned at a lower rating than oil.

Pulverized coal, the third of the fuels being burned in suspension, is operated with a higher average excess air and a lower rate of B.t.u. absorption per cubic foot of furnace volume. This difference, in comparison with oil, is largely due to the trouble caused by the fluxing of refractory by the ash in the coal, and also due to the fact that it is not atomized as finely as is oil from a good burner, even though it is pulverized to 200 mesh. It is therefore evident that the larger particles require a larger furnace and longer period of time for best results.

With fuels burned on underfeed and chain-grate stokers the average results indicate higher excess air, with rates of combustion approaching those of oil, yet individual installations of modern equipment show lower excess air where water-cooled furnace construction has solved the refractory problem satisfactorily, or where coal having a higher-fusing-temperature ash is used.

It is believed that Figure 3 will enable different plant operators to study their own performance and see how it plots up with the same type of equipment, and perhaps this basis of comparison will assist operators and manufacturers of different power-plant equipment in their endeavor to reach the goal we are all striving so hard to attain.

There is another point of comparison that is not brought out with definite numerical values in Figures 2 and 3, namely, the relative importance of the limiting factor where it relates to CO and unburned fuel.

RELATION BETWEEN EXCESS AIR AND CO LOSS.

Figure 4 represents the relation between excess air and CO loss and applies fairly well to either underfeed or forced-draft chain-grate stokers. Curve A represents theoretical conditions with perfect mixture and a reduction in excess air until the theoretical amount is reached. Any further reduction will of course result in unburned gas, usually in the form of CO—and perhaps some hydrocarbons also, thereby increasing the rate of heat loss approximately 15 times as fast for each per cent of deficiency in air as it does for each per cent of excess air.

Since a perfect mixture cannot be obtained, the curves for different furnaces designated as B, C, and D show the points at which CO begins to appear, and the rate of its increase with decreasing percentages of excess air.

The lower set of curves showing the total losses due to excess air, unburned gas, and latent heat indicate the most efficient percentage of excess air to be striven for. For furnace B the most efficient point is with about 12 per cent excess air; for furnace C it is 30 per cent excess air, while with furnace D 70 per cent excess air is the most efficient point; that is, provided no other factors except CO and excess air are considered.

The question of unburned coal over the end of chain-grate stokers often holds a close relation to excess air, and this should be studied separately.

Carbon loss should also be carefully checked on other types of stokers, as well as on pulverized coal, where the carbon is likely to escape in the flue dust and may sometimes reach serious proportions even without showing objectionable black smoke.

Whenever smoke of any consequence does appear in gases from pulverized coal, it is indicative of a considerably greater loss than when smoke results from the combustion of other types of fuel.

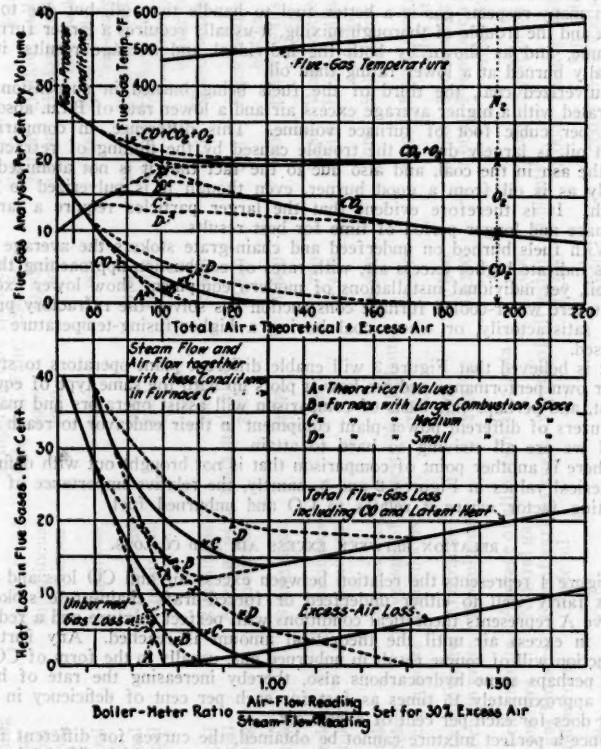

Flue-Gas Analysis, Per Cent by Volume
Flue-Gas Temp., °F
Gas-Producer Conditions
Flue-Gas Temperature
CO+CO₂+O₂
CO₂+O₂
N₂
O₂
CO₂
CO
Total Air = Theoretical + Excess Air
Steam Flow and Air Flow together with these Conditions in Furnace C
A = Theoretical Values
B = Furnace with Large Combustion Space
C = " " Medium " "
D = " " Small " "
Heat Lost in Flue Gases, Per Cent
Total Flue-Gas Loss including CO and Latent Heat
Unburned Gas Loss
Excess-Air Loss
Boiler-Meter Ratio Air-Flow Reading / Steam-Flow Reading Set for 30% Excess Air

FIG. 4.—COMBUSTION DIAGRAM FOR FUEL BURNED ON STOKER.

[McDowell Co., W. Va., coal. Proximate analysis: Moisture, 2.19; volatile, 13.91; fixed carbon, 75.25; ash, 8.65. Ultimate analysis: S, 0.57; H, 4.45; C, 80.69; N, 1.19; O, 4.45; B.t.u., 13,995 (lower, 13,607).]

THEORETICAL TEMPERATURES OF COMBUSTION WITH DIFFERENT PERCENTAGES OF EXCESS AIR.

In discussing Figure 3 it was pointed out that under certain conditions it was necessary to increase the percentage of excess air with rating in order to minimize difficulties and expenses in connection with refractory. There is an increase in theoretical temperature of about 200 degrees for each 10 per cent reduction in excess air. Therefore the percentage of excess air is a very important factor in controlling furnace temperatures for a given set of furnace conditions.

This point is of course well known and carefully followed in metallurgical work, where temperatures receive first consideration. High furnace temperatures are also of great importance in connection with the efficient combustion of fuel, and are quite analogous to the greater efficiency obtained from higher steam pressures and higher voltages in the transmission of energy in the form of steam and electricity. Heat is transmitted from the furnace to the heating surface more efficiently with higher furnace temperatures than it is with low heat potentials.

It is seen that an increase in flue-gas temperature occurs with an increase in percentage of excess air. This is true, and is due to the higher velocity of the gases through the boiler and the lower rate of heat transfer by radiation when the initial temperatures are lower. Preheated air is an important factor today as it is so widely used in our modern furnaces.

The importance of excess air in controlling furnace efficiency and operating conditions has been pretty well brought out in the foregoing data and references. The next question is, What is the best method to determine the percentage of excess air both for test and investigation work, as well as for the daily operating guide of the fireman?

It is rather lamentable that so many people consider CO_2 and excess air as synonymous, and it is safe to say that the majority today refer to combustion conditions by stating the percentage of CO_2 obtained rather than by referring to the percentage of excess air. Most people have stopped with this one figure, oftentimes feeling that it represented the whole story of combustion efficiency. The author would like to emphasize that this is not the case, for the percentage of CO_2 desired with any given fuel depends upon the chemical composition ranging from 7½ per cent with coke-oven gas to 22.8 per cent CO_2 with blast-furnace gas for 20 per cent excess air. Even with different kinds of coal the CO_2 varies more than 1 per cent for the same excess air. It is becoming more and more common to change back and forth from one kind of fuel to another, such as coal, oil, or gas, and oftentimes to burn a mixture of gas and pulverized coal or other fuels in the same furnace simultaneously. It is therefore quite important to analyze the flue gas for more than CO_2 to determine the percentage of excess air accurately.

For all fuels except blast-furnace gas the following formula is substantially correct for figuring the percentage of excess air:

$$\text{Excess Air} = 100 \times \frac{O_2}{0.264N_2 - O_2}$$

The percentage of oxygen is a much closer indication of excess air than is the percentage of CO_2, but even oxygen as a guide varies with different fuels depending upon the hydrogen-carbon ratio, so that it alone is not a true indication of excess air.

RELATION BETWEEN EXCESS AIR AND $CO_2 + O_2$ IN DIFFERENT FUELS.

In a carbon fuel having no available hydrogen the CO_2 plus oxygen is equal to 20.9 per cent when there is no CO present. However, in fuels which have available hydrogen there is a certain amount of oxygen which combines with the hydrogen, forming water vapor, and thereby disappears from the gas analysis as ordinarily made. This shrinkage increases, so to speak, with the percentage of available hydrogen in the fuel, so that with oil, natural gas, and coke-oven gas the total CO_2 and oxygen is considerably below the figures with which we are familiar in analyzing flue gases from bituminous coal.

It is best for one to be on his guard in checking over gas analyses from various kinds of fuel to make sure that the samples are representative and the analyses are being properly made. While the curves given here are labeled "Oil," "Natural Gas," "Bituminous Coal," etc., they are not to be taken as truly representative of all such fuels, because any particular fuel of a given class having a different ultimate analysis, or rather a different ratio of available hydrogen to carbon, will give different results on this gas-analysis basis.

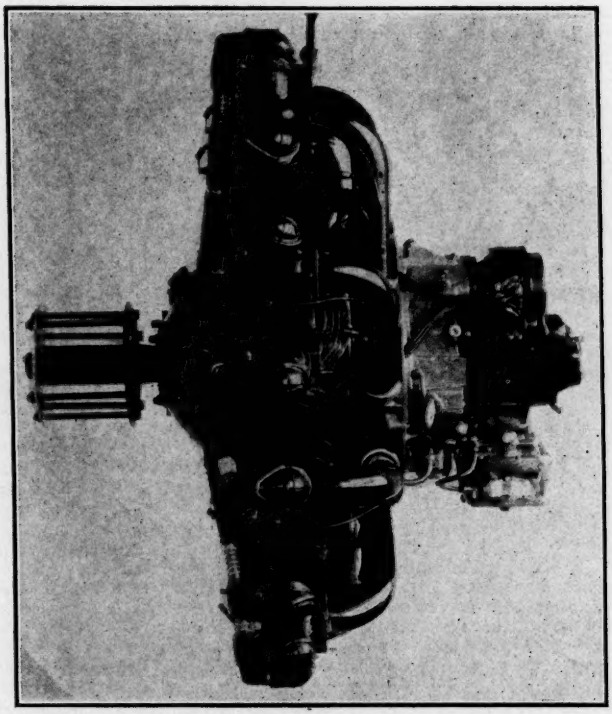
For example, in the burning of a coal having 79.86 per cent carbon and 4.04 per cent available hydrogen, the available hydrogen-carbon ratio is 0.0506 per cent. If a flue-gas analysis from this shows 15.16 per cent CO_2, 3.71 per cent oxygen, and 81.13 per cent nitrogen, then the percentage of excess air is 21.

Supposing, however, that a gas analysis from this same coal showed 14.96 per cent CO_2, 3.72 per cent oxygen, and 81.32 per cent nitrogen; the excess air would figure the same, namely, 21 per cent, but following this through from the curve would show a ratio of available hydrogen to carbon of 0.0560. If we assume that all of the hydrogen in the fuel was burned, which is very likely to be true, then dividing the percentage of available hydrogen, 4.04, by the 0.0560, gives 72.14 as the percentage of carbon in the fuel that was actually burned and which was shown up in the gases as CO_2. This would indicate that 7.72 of the 79.86 per cent carbon in the fuel was lost in the form of coke going to the ashpit, or as solid particles of carbon escaping in the flue gases. It is obvious that with the burning of pulverized coal and the desire to determine the carbon loss more accurately, careful flue-gas analyses must be made in order to aid in detecting this carbon loss without the necessity of running complete boiler tests and thus determining it through the medium of unaccounted-for loss in the heat balance. It is true that this will require more accurate gas analyses than we are accustomed to obtain with the Orsat apparatus. However, it is evidently worth while and should be given careful consideration.

Another point to be given careful consideration in this study of gas analyses is that of stratification of the gases. Gas samples taken from a stratum in which an abnormally large percentage of the hydrogen is being burned will give a lower total $CO_2 + O_2$ than is normal for the fuel. The extreme case may approach that of coke-oven gas. On the other hand, if the sample is taken from the rear end of a chain-grate stoker, the total $CO_2 + O_2$ will run very high, approximating that of coke.—"Mechanical Engineering," July, 1926.

THE PRATT & WHITNEY WASP ENGINE.

The new Pratt & Whitney Wasp engine recently completed its first tests at Anacostia, Washington, D. C., under the auspices of the Navy Department. The tests were made in the Wright Apache, a remarkably clean single-



A Side View of the Pratt and Whitney Wasp Engine Giving an Excellent Idea of the Cleanness of the Front of the Engine and Showing Clearly the Arrangement of Equipment in the Rear. The Eclipse Engine Starter and One of the Scintilla Magnetos Can Be Seen.

place plane designed to meet the requirements of the Navy shipboard fighter type, and for the installation of an air-cooled radial engine.

Great interest has been attached to the ability of an air-cooled engine to successfully compete with the present standard water-cooled types, particularly as regards high speed. It has long been recognized that, because the air-cooled engine installed in an airplane is lighter than its water-cooled competitor, the climb of an airplane so equipped would be superior, but, on account of the head resistance of a radial type, and also the belief that air-cooled engines were not as efficient as water-cooled types, many have felt that superiority in speed would go to the airplanes with a water-cooled engine. Following the results of the Navy's tests, it is interesting to note the comment as given out by Comdr. E. E. Wilson, head of the Engine Section of the Bureau of Aeronautics, who stated: "The Wright Apache, fitted with the Pratt & Whitney Wasp engine, has exceeded the high speed of the present standard water-cooled pursuit planes. Its climb in ten minutes was better than that of the standard machines fitted with water-cooled engines by more than sixty per cent."

It is believed that the Wasp engine has set an entirely new standard for radial engine efficiency. Exhaustive tests under the supervision of the Navy Department have established a rating for this engine of approximately a pound and one-half per horsepower, and it has shown its ability to develop power in excess of this figure. For the first time, a radial type engine in the 400-horsepower class compares more than favorably with the efficient water-cooled types of engines on a dry weight per horsepower basis. Its weight installed in an airplane is, of course, considerably less than the water-cooled types on account of this, together with the saving of water, radiator, etc.

The Wasp is a nine-cylinder air-cooled fixed radial type. The bore and stroke are 5.75 inches, giving a displacement of approximately 1300 cubic inches. The compression ratio is 5.25, and the engine operates entirely satisfactorily on its standard rating with domestic aviation gasoline. Its normal high speed is 1900 R.P.M., and it is being conservatively rated at 400 horsepower. The dry weight of the engine, including accessories, except the starter, is 650 pounds.

EMBODIES MANY NEW FEATURES.

The Wasp has obtained unusual characteristics because of certain features entirely new in radial design. All of the power transmitting parts are extremely rugged and substantial, and exhaustive tests have shown the engine to be remarkably dependable in spite of the low weight per horsepower.

The accompanying photograph of the unmounted engine shows the front of the engine to be entirely clean. This provides good opportunity for proper cowling. All of the accessories and drives are conveniently arranged at the rear of the engine, including the two Scintilla magnetos, Stromberg carburetor, oil and fuel pumps, provision for synchronizer drives, Eclipse inertia starter, and even provision for a generator. All of these accessories are entirely enclosed by the fuselage, and are easily accessible through inspection doors on either side of the fuselage forward of the fire wall. This arrangement not only makes for extreme accessibility, but, even more important, entirely protects these accessories from the elements.

The valve gear is completely enclosed, including the push rods, valve springs, and the rocker arms, which latter are carried on ball bearings. To accomplish this, each rocker arm is carried in a housing which is integral with the cylinder head. The tubes enclosing the push rods are readily removable. Provision is made for removing the valve springs and valves without disturbing the rocker arms.

Departing from conventional radial practice, the main crankcase consists of two symmetrical sections divided on the center line of the cylinders and united by nine through bolts, one between each cylinder. By this arrangement each main crankshaft bearing is supported directly in the main case. Extreme smoothness is secured by the use of a rotary induction system which is built into the rear of the engine. A certain amount of supercharging is also provided for with this arrangement. This system was developed in cooperation with the General Electric Company.

Again departing from conventional radial practice, it is interesting to note that the master connecting rod of the Wasp has a solid big end. The split type master rod has long been a fundamental weakness of all large displacement radial engines, and at the same time has made it impractical to operate smaller displacement radials at high revolutions. The Wasp has apparently overcome this weakness by the employment of the solid rod. This construction permits, without penalty of additional weight a master rod sufficiently rugged to withstand both high power loading and extremely high revolutions. A divided crankshaft is used with this rod construction. The forward or transmitting portion of the shaft is solid, while the rear or accessory drive member is splined and bolted to it, forming a shaft assembly as rigid and strong as a single piece shaft.

In general, much ingenuity has been shown in the elimination of unnecessary weight. To this end a great deal of duralumin is used throughout the engine, and many refinements in details enter into the remarkably low weight of the completed engine.

George J. Mead, vice-president of the Pratt & Whitney Aircraft Company, who directed the development of this engine, predicated its design upon the desire of the Navy Department for an efficient air-cooled engine for use in shipboard fighters. The Navy have frankly stated their preference for an air-cooled type for this use, but it has been necessary to develop greater efficiency in radial types in order to successfully compete with the very efficient and proved water-cooled types now in use. Mr. Mead stated: "It has been our aim to develop a 400-horsepower radial type of engine whose weight per horsepower compares favorably with the most efficient water-cooled types now being used for pursuit planes. It is a fact that the Wasp, on a basis of dry weight, is lighter than the present standard water-cooled engines, and, based on the official Navy tests, it is capable of developing as much or greater power. Of course, its installed weight in the airplane is considerably less than that for the water-cooled types. The flight tests at Anacostia demonstrated that a fighter equipped with the Wasp air-cooled engine could 'out-perform' the present standard water-cooled planes. This seems proof of the soundness of the Navy's policy for the past several years of encouraging air-cooled developments for their requirements."

It is interesting to note that the first engine of this type has now been operated for more than 250 hours of testing, nearly all at full throttle. After more than 100 hours of preliminary testing, the same engine was placed on official Navy fifty-hour test, which it completed exactly in accordance with a pre-determined schedule without stops for any cause, and without the replacement or adjustment of a single part, not even excepting spark plugs. Shortly after this test the following interesting statement was given out by the Office of the Secretary of the Navy:

"The Secretary of the Navy announced today the award of a contract to the Pratt & Whitney Aircraft Company of Hartford, Connecticut, for a number of their new Wasp engines. This is the engine which has recently completed two standard type tests under the supervision of the Navy Department with surprising results. The details of construction of this engine

were not obtainable from the Navy Department for the reason that it is considered a highly secret development. It was learned, however, that the Wasp is a radial air-cooled engine of nine cylinders, developing more power than the Liberty on over 150 pounds less weight. Conservatively rated, the engine is reported to weigh less than 1.5 pounds per horsepower, or less than any known air-cooled engine of similar power here or abroad. It incorporates a number of novel design features which give it a surprising performance both at sea level and at altitude. It is the result of an intensive development and incorporates advanced engineering design with the precision workmanship for which the Pratt & Whitney Company is well known throughout the world.

"A number of new airplanes for shipboard use are being built around this engine. It is expected that some of these will be in the air at a very early date. There is every assurance that, as a result of this development, the fleet will soon have aircraft whose performance is far in excess of anything available elsewhere. If the Five-year Building Program now before Congress is approved, funds will be available to equip the fleet immediately with these improved types."

In regard to the present plans of the Pratt & Whitney Aircraft Company, it is interesting to note the statement just given out by F. B. Rentschler, the president of the company, as follows:

"We feel greatly pleased with the performance to date of our Wasp. At a recent meeting of our Board of Directors we were authorized to proceed immediately with expenditures for new machinery and equipment aggregating more than $300,000. With this equipment added to our present experimental shop we believe that our manufacturing facilities for aviation engines will be unsurpassed.

"The Board of Directors of our company has been recently expanded to include Charles F. Kettering, vice-president in charge of research and engineering of the General Motors Corporation, and a director of that company, and William B. Mayo, chief engineer of the Ford Motor Company, and who has also directed the interesting and successful aviation operations of that company.

"All of our people have great confidence in the future of aviation, both from a military and a commercial standpoint. It is a source of real satisfaction that sound and established business heads are anxious and willing to lend their support to the continued development of aeronautics."—"Aviation," May 31, 1926.

THE HILL TAILLESS AEROPLANE.

Although the number of accidents occurring to aeroplanes in flight due to purely aerodynamic reasons is fortunately small, such as do happen usually result from loss of control while the machine is stalled. This occurs when the angle of incidence exceeds the critical value, usually about 18 degrees, the lift at greater angles diminishing as the angle of incidence increases and the normal controls becoming ineffective. A skilled pilot can get the machine out of a stall if he has sufficient head room, but when stalling occurs near the ground especially in bumpy weather, it usually results in a serious accident, since the time and height required to carry out the maneuvers necessary for a recovery are insufficient. Several inventors, from time to time, have endeavored to get over this difficulty, and the problem has been recently tackled by Captain G. T. R. Hill, M. C., who gave a particularly interesting account of his work and the results he has obtained, in a paper read before the Royal Aeronautical Society on the 22nd instant.

Captain Hill commenced his work some three or four years ago with the idea that the center of pressure of the wing must be stationary at all the attitudes required for a given speed range, and, to secure this, decided to employ a swept-back wing with the incidence washed out towards the tips. Movable extensions were to be provided at the wing tips for control purposes, these members functioning as ailerons and as elevators, respectively, when used independently or together. The wings were also intended to be tapered in plan mainly to reduce the chord of the controllers, but tapering also has the effect of reducing the air damping and inertia about the longitudinal axis, as well as of reducing the structure weight from the fact that there is less bending moment on the wing truss at the center. It follows from the fact that the center of pressure of the wing is stationary and coincident with the center of gravity of the machine, that the position of the controllers for trim at any angle of incidence would be along wind, and there would be no force acting on them in straight flight. Their full angular range would thus be available for control purposes even when the main wing was stalled, since the controllers would not be stalled. To steer the machine, Captain Hill decided to use vertical surfaces, which may still be called rudders, mounted below the wing surface on each side near the extremities of the fixed part.

At this stage, *viz.*, in the spring of 1923, his proposals, which were regarded as a natural development of the earlier tailless aeroplanes, of which that designed by Lieut. J. W. Dunne may be mentioned in particular, were submitted for the consideration of the Aeronautical Research Committee, but Capt. Hill decided himself to construct a light aeroplane on the lines indicated above, in order to test out his theories under practical flying conditions. The construction of a monoplane, with semi-cantilever bracing and having a very light loading, was commenced, and during the progress of the work model experiments were carried out at the Royal Aircraft Establishment, at the instance of the Aeronautical Research Committee. Certain modifications to the original design were made as a result of these experiments, and the machine was eventually completed and tried, first as a glider, in December, 1924. Although carried out under unfavorable weather conditions, the gliding trials were considered satisfactory, and, with further assistance from the Air Ministry, a body was fitted, and a Bristol Cherub engine installed.

The machine, as finally completed, is illustrated in Figures 1 and 2. The illustrations show the machine in flight, and, of these, Figure 1, which was taken with the machine almost overhead, illustrates the plan form of the wings most clearly. Figure 2 is a side view of the machine flying at a considerable angle of incidence. The area of the main plane is 223 square feet, and the overall span is 45 feet while that of the fixed part only is 30 feet. The section used is based on that known as Airscrew 4, described in Reports and Memoranda No. 322, but, as a result of the model tests, the trailing edge was reflexed upwards at all points by 0.015 chord, and the top camber was reduced somewhat towards the wing tips. The sweep back of the leading edge of the main plane is 31 degrees and that of the trailing edge 14 degrees. The wash out of incidence is 3 degrees at the strut attachment, increasing by a further 3 degree to the tip, and there is no dihedral angle. The controllers, which have an area of 55 square feet, are of the T. P. 3 section, except near the extreme tips, which are reduced to thin edges, and lead balance weights, visible in Figures 1 and 2, are fitted on arms projecting forwards to secure a static balance. A variable gear ratio is employed between the pilot's control stick and the controllers, to give a low ratio when flying at high speeds and vice versa. A spring trimming device is also fitted to give any desired force

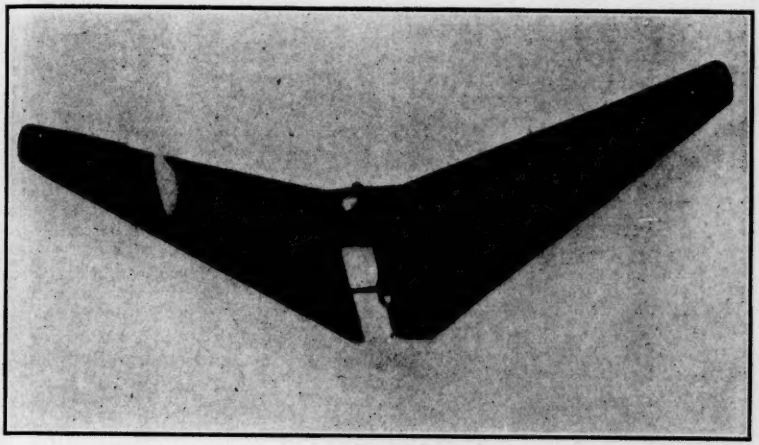


FIGURE 1.

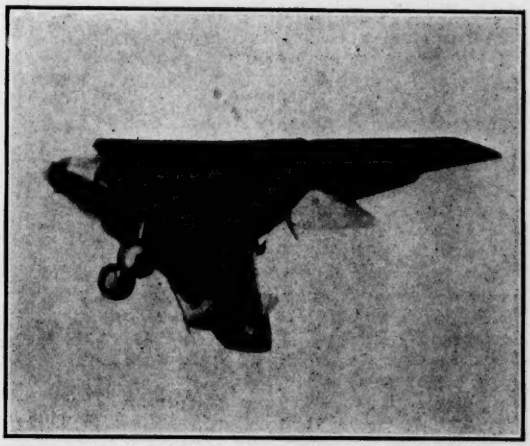


FIGURE 2.

on the control stick within the range + 15 pounds or — 15 pounds, this device replacing the usual tail-adjusting gear.

An air brake is formed by the operation of both rudders simultaneously, and this serves to give a gliding angle twice as steep as the normal. When both rudders are in use as an air brake, however, rudder control can still be obtained. The rudders, which are 13 square feet in area, are operated by a rudder bar of the usual type, but a special handle is provided for controlling their use as an air brake. Other levers are used for operating the trimming gear and the ground brake, the latter acting on the rear wheel of the undercarriage. The two front wheels of the latter are mounted on a common axle, and two legs pass up from this through holes to the inside of the body, where they are anchored by shock-absorber cords. At the rear of the undercarriage is a single metal wheel mounted in a fork, which is connected to the rudder bar for steering the machine on the ground. A locking device is also fitted to the wheel so that it can be used as a brake in landing, as previously stated. It may be mentioned that about one-third of the weight of the machine is carried on this wheel, as compared with about one-sixth of the weight carried by the tail skid of an ordinary aeroplane; the arrangement thus quickly brings the machine to rest after landing. The body of a tailless aeroplane car, of course, can be made of lighter construction than that of an ordinary machine, since, in the former, the body is not required to withstand heavy bending moments due to the tail plane and tail skid. In the Pterodactyl, as Captain Hill has called his machine, the body provides accommodation for the pilot and one passenger, and it is built up with a special light balsa ply-wood, the weight of which is only half that of standard birch plywood. The engine drives a pusher screw at the after end of the body and the engine mounting consists of four steel tubes, with ball-and-socket end fittings braced with tie rods. The whole weight of the mounting is only 3½ pounds, but it has been found to be quite effective in all respects.

Upwards of 20 flights have now been made with the machine, and some particulars of the results obtained will doubtless be of interest. The maximum speed of the aeroplane near the ground has been accurately determined as 70 miles per hour with a total load of 658 pounds, the wing loading then amounting to 2.95 pounds per square foot, and the horsepower loading to 20 pounds. The minimum speed, as measured by the air-speed indicator, was between 24 miles per hour and 25 miles per hour, but an addition of about 4½ miles per hour to this reading was found by wind channel tests to be necessary, bringing the actual minimum speed to about 29 miles per hour. This latter figure compares reasonably with the figure of 31½ miles per hour as calculated from the maximum lift coefficient measured on the model. By adjusting the spring trimming gear, the machine could be flown without touching the control stick at any speed, and was also found to be stable, both longitudinally and laterally, throughout its whole speed range. Owing to its light wing loading, however, it was rather sensitive to bumps, more particularly in the fore and aft direction, and one of the least satisfactory features noted in connection with the controls operated by the stick was the excessive amount of inertia existing in the system. This defect may, however, be reduced in later designs by lowering the gear ratio between the stick and the controllers, by reducing the area of the latter, and by reducing the inertia of the balance weight. Apart from the inertia effect, the fore and aft control was sensitive, powerful and reasonably light, and similar remarks apply to the lateral control.

No definite stalling point was found, although there was a definite minimum speed; after this speed is reached, the more the control stick is pulled back the faster will the machine sink. Under these conditions, therefore, the machine may be said to be stalled, since the angle of incidence is greater than

the critical angle. Good control, however, could still be obtained even in bumps, and it has been found possible to unstall without diving for the purpose of gaining speed. Lateral control, when stalled, is effective, but is sluggish owing to the low air speed, and the rudder control, under the same flight conditions, is quite adequate to maintain direction and to carry out gentle turns. With regard to the air brake it may be mentioned that when this is applied, the machine slows up and the nose must be put down to maintain the speed, but owing to the inherent stability of the aeroplane this will occur automatically if the control stick is not touched. With the air brake on, the machine glides comparatively steeply at about 40 miles per hour, and it becomes a very simple matter to judge the approach to land. Landing is extremely easy, and the machine steers well when taxying on the ground. Some pitching is, however, experienced when running over rough ground, due to the short wheelbase, and the pilot feels this motion acutely owing to his position some distance in front of the wheels. In Captain Hill's opinion, however, there is no danger from this cause.

It still, of course, remains to be proved that the essential features incorporated in the design of Captain Hill's tailless monoplane will prove equally satisfactory when applied to a more heavily-loaded machine of the biplane type, such as could be used for air transport. There is no obvious reason why this should not be the case, and any means of rendering flying safer and more reliable are certainly worthy of careful consideration. In any case it would appear that a light machine of the type described, when the few comparatively minor drawbacks above referred to have been eliminated, would be well suited to the requirements of the private user.—"Engineering," April 30, 1926.

PRIMARY BATTERY CONVERTS COAL INTO ELECTRICITY.

Direct conversion of coal into electricity without boilers, engines or generators, has long been a dream of power engineers. Often they assume that any such development must involve the discovery of some new principle. As a matter of fact the fundamentals of possible methods have been known for years, although nothing has been done on a commercial scale. There is much food for thought on this subject in the following excerpt from an editorial in the June issue of "The Power Engineer" (London).

"It is possible, however, that working along totally different lines—eliminating the wasteful steam boiler and engine with its even more wasteful condenser—a process of converting the combustion energy of coal into electrical energy can be found, and in an article entitled 'The Problem of the Fuel Cell,' contained in the Royal Technical College (Glasgow) Metallurgical Club Journal, Ulick R. Evans, M. A., indicates how this may be done with an efficiency—coal to kilowatts—of no less than 95 per cent. The process which, after all, involves nothing fundamentally new, is to use coal as the consumable pole of a primary battery in much the same way as zinc is employed at the present time. Carbon takes its place in the electrochemical series wherein any two substances placed in acid (or some other reagent) yield an electric current, and since coal contains a large although varying percentage of carbon, it is feasible that coal in conjunction with some other suitable substances could form the basis of a primary cell. Already a certain amount of research work has been carried out along these lines, and the author, after reviewing what has so far been accomplished, makes the suggestion that by intensive investigation it might be possible to develop a battery which would yield electrical energy on a commercial scale.

"The best form of cell using coal as the consumable element is probably that introduced by Jacques in 1897 and improved by Taitelbaum in 1911, in which the cathode is of passive iron and the anode of coal, lignite or charcoal the electrolyte being fused caustic soda. But whereas this cell gives 0.6 volt when balanced on a potentiometer, as soon as it is made to furnish a current the electric motive force drops owing to polarization. Hence, the author suggests that a better scheme is to employ the coal to generate water gas, producer gas or some other gaseous fuel, and use this gas in the cell. An example is a cell in which one pole consisted of nickel saturated with hydrogen and the other of silver saturated with oxygen, the electrolyte being of fused borax. Carbon monoxide was employed as the regenerative agent and the cell gave slightly less than one volt when balanced. Other combinations have been tried notably one in which the electrodes were packed in porous tubes, the positive one consisting of magnetite granules (Fe_3O_4) and the negative one of iron wire sinkings, the electrolyte being fused sodium and potassium carbonates.

"If this type of cell is to be developed on a commercial scale, it is not unlikely that the components of the cell will have to be placed closely together and thus a refractory material will be required to form a diaphragm. At present the fuel cell yields about 300 watts per cubic meter, so that the size of a battery of any appreciable power can be easily imagined, but the author makes the valuable suggestion that as a step in the right direction it might be possible to develop a primary battery using coal gas of sufficient capacity for charging accumulators. Such a battery would undoubtedly have a good chance of demonstrating the possibilities of the fuel cell as an economical means of producing electrical energy, and to those who are impressed with the idea of the fuel cell it may be mentioned that the author gives a brief summary of the literature of the subject from whence full particulars of the work so far accomplished can be obtained."—"Power," June 29, 1926.

SYMPOSIUM ON DESIGN OF WELDED JOINTS.

Papers presented at the technical sessions, held April 22, of the American Welding Society, were planned to form a symposium on the subject of design of welded joints in several industries. Data presented included stresses, accessibility and position, technique, type of joints, materials, thermal considerations and tests. Most of the papers were preprinted in the April issue of the "Journal of the American Welding Society." Brief abstracts of the papers are given below.

STRUCTURES DESIGNED FOR WELDING.

Emphasis was placed on the necessity of designing for welding, rather than using riveted design and substituting welding for riveting, in a paper on the "Design of Oxwelded Equipment," by E. E. Thum, engineer, Linde Air Products Co., New York. An engineer would not be satisfied to take a design for a riveted truss and merely note on the drawing: "Substitute welding for riveting" and expect to get satisfaction, said Mr. Thum.

Some advantages of welding in connection with the construction of a lime kiln 125 feet long and 8 feet in diameter were outlined. Three of these kilns, two of which were riveted and one oxy-acetylene welded, were placed in operation late in 1923. Riveted kilns of this size have usually been made of $\frac{5}{8}$-inch plate, with butt strap joints: double riveted on logitudinal seam and triple riveted on circumferential seams. The metal in such a design, it was

said, is not working to anywhere near its capacity. Results of tests, made after two years' operation of the kilns, to determine the reason for the excess of metal required for the shell were given. This investigation was claimed to show that the riveted kiln must be made of the heavy plate, and also that there is no reason why a welded shell should be as thick as a riveted shell. It was indicated that a ½-inch plate might have been used for a welded kiln of this size, which, together with other advantages, would bring the cost materially below the riveted construction.

Savings in drafting room expense—in the time of draftsmen, tracers and checkers—by the use of welded design, were also discussed, the design of a skew hopper for a dust-collecting system being used to show the advantages in this particular of welded design.

Principles which should control the design of oxy-acetylene welded joints were included in the paper. A table showing the ultimate tensile strength, with welding rods of various material, was given, and also figures for the strength of reinforced welds, based on the thickness of the plate rather than the actual thickness of the weld.

POSSIBILITIES OF WELDING IN PIPING DESIGN.

Engineering principles involved in welded piping design, and the possibilities of welding in piping when used under proper conditions were dealt with in a paper by L. J. Sforzini, engineering and maintenance department, Eastman Kodak Co., Kodak Park, Rochester, N. Y. The title of the paper was "Welding in the Design of Industrial and Power Piping."

High-pressure steam, low-pressure steam, refrigerating fluid, air, water and general fluid piping were discussed in the order given, the discussion being prefaced by an outline of the advantages and disadvantages of the four principal types of non-welded joints used in pipe work. Mr. Sforzini said that he was not of the opinion that everything in a piping system should be welded to the exclusion of all other methods; but that every piping system should be studied by itself to determine whether welding can be used to advantage.

In conclusion it was said that "the welded pipe joint is a satisfactory joint, easy to make, comparatively cheap for large size pipe or for high pressure pipe, is a time saver, gives the designer freedom in piping layouts, and once made tight, stays tight. It saves in insulation cost. Its use makes possible certain economies in the industrial power plant pertaining to steam distribution, in permitting concentration of power and heat production. Low pressure steam distribution can be made in cheap steel mains of large size, which can be added to at low cost." It was further stated that welded piping installed under proper supervision is fully as reliable as piping with any of the other ordinary joints. Welding was stressed as an important tool for the piping designer, who should understand both its possibilities and limitations.

METHOD OF MAKING PRESSURE TANK OUTLINED.

Steps in the design and manufacture of an electrically welded tank for use as an air or steam receiver under 250-pound working pressure were outlined by L. H. Burkhart, chief engineer, Struthers-Wells Co., Warren, Pa., in a paper on the "Design and Manufacture of a Horizontal Cylindrical Pressure Tank."

The proposed tank was 48 inches in diameter, 15 feet long on the cylindrical shell, and had the heads dished outwardly. A manhole and the required openings, fitted with nozzles or screwed flanges, were part of the structure. The tank is to be made by fusion welding (metallic arc) through-

out, and is to be according to the unfired pressure vessel code, using a unit stress of 8000 pounds for double V-welded joints. The method of determining the thickness of the shell and heads, selecting the most economical size of plate and the method of preparing the shell and heads for welding were outlined. The method of welding was described in detail, the assembly of parts and make-up of the welds being shown by means of line drawings.

WELDING OF BILGE BARRELS AN EXACTING OPERATION.

The use of welding in the manufacture of bilge barrels was described in a paper on "Design of Welded Tanks for Tanks and Containers," by A. C. Vick, plant engineer, Detroit Range Boiler & Steel Barrel Company, Detroit. Because of Mr. Vick's absence presentation of the paper was by title. The Detroit company has employed welding in various forms for many years. Difficulties were met with in applying welding to the manufacture of the bilge barrels, these difficulties being finally overcome as outlined in the paper.

The "bilge" barrel gets its name from its shape. The shell of the barrel is made from sheet steel ranging in gauge from No. 12 to 14. This sheet is carefully resquared on the two edges which butt together when the sheet is rolled and which forms the joint to be welded. After welding, the shell is placed on an expander, which forces it to take the exact shape of the mandrel. This operation is known as "bilging."

The stringent demand put upon the welding may be noted from consideration of the amount of bilging involved. In bilging a 55-gallon bilge barrel shell, the diameter is increased from 21 to 24½ inches at the bilge, the ends remaining 21 inches in diameter. The diameters at intermediate points are governed by the height of the segment at that particular point. The expansion of the 3½ inch at the bilge effects an elongation of 11 inches, which is distributed over 66⅓ inches, the circumference of the original cylinder. This is an elongation of 0.166 inch per inch of circumference. The elongation reduces thickness approximately 0.007 inch, which in the case of a No. 13 gauge shell, is a reduction in area of 7½ per cent.

Various changes in the methods of welding these barrels, devising of jigs, etc., since beginning the welding of this production in 1914, are outlined in the paper. With steel of proper analyses and annealing the percentage of "breakers" was said to be small. Welds made on 13-gauge stock were cut in 1½-inch wide strips at right angles to the weld and were tested in a testing machine. The strips broke at 1800 pounds at a point ½ inch from the weld, this being stressed as indicating that with good steel the welds could be made stronger than the original material. An important requirement of these barrels is that they be airtight and the tests made to determine air tightness were briefly outlined.

WELDED JOINTS IN SHIP CONSTRUCTION.

Practically every form of welding can be used advantageously in the repair and construction of ships, according to J. W. Owens, director of welding, Newport News Shipbuilding & Dry Dock Co., in his paper on the "Design of Welded Joints in Ship Construction."

As automatic arc welding machines permit of welding the butt joint at a high rate of speed, and thereby minimize thermal stresses, it might be possible in the future, said Mr. Owens, to use this joint, unstrapped, provided the carbon content of the plate be kept low, preferably below 0.15 per cent, and the joint properly reinforced with weld metal. Another basic consideration might be the deposition of weld metal of a higher tensile strength than that of the base metal, or weld metal, which has as high ductility as can be secured with the hydrogen arc.

Thermit welding is, in Mr. Owens' opinion, preferable to smith welding for the construction of new stern frames and the frames of large rudders. The application of gas welding in shipyards to the construction of tanks, pipe fittings, welding of pipe lengths without the use of flanges and in the making of metal furniture, is considered good practice. Metal arc welding can also be used for this work. The butt joint is preferred for gas welding. Gas welding is used almost exclusively for the welding of non-ferrous metals and cast iron when readily preheated.

In discussing technique, Mr. Owens said that metal arc welds should in general be made in one layer, although the use of multiple layers is frequently necessary. In testing piping systems, tanks and watertight bulkheads existing tests for riveted joints, with or without the hammer blow test, are desirable. As the testing of each joint of the ship's structure is impracticable, reliance will have to be placed on properly designed joints, competent supervision, certification of all welders and the use of carefully selected filler material. Mr. Owens' company is taking steps to certify all of its welders and to issue standard practice sheets covering design and technique.

The relative merits of various formulas in use for determining the internal pressures in a cylindrcal shell of closed end vessels were discussed by S. W. Miller, consulting engineer, Union Carbide & Carbon Research Laboratories, Long Island City, N. Y., in a brief address on the "Design of Welded Joints."

In a paper on "Arc Welding in Hydrogen and Other Gases," P. Alexander, Thomson Research Laboratory, General Electric Co., Lynn, Mass., described the new method of arc welding in a hydrogenated atmosphere. An outline of the methods described by Mr. Alexander was published in "The Iron Age" of April 8, page 989.—"The Iron Age," April 29, 1926.

STUNTS WITH THE WELDING TORCH.

By L. A. Cowles.

The welding torch may be used to caseharden. The steel is brought up to a white heat, the oxygen shut off and the acetylene played on the metal. Repeating this process adds carbon, and a thin, hard surface is the result.

Welding upside down is quite feasible, although many think that the piece to be welded must be in such a position that the weld is made down. In welding around a pipe, it is necessary to weld up. With a little practice it will be found that this is not at all difficult. In welding pipe, start at the bottom center and weld around to the top center. Then repeat on the opposite side. On large pieces two or more welders work simultaneously to minimize contraction strains.

When welding thin spots in boiler tubes and pipe, a hole through the metal often will burn through, and the welder, in attempting to close it, may find he is only making it larger. To remedy this, stick one end of the filler rod to one side of the hole. Lift the torch and then stick the filler rod to the opposite side. Keep bridging across with the filler rod until it is possible to go back over and weld the bridge and sides together, working toward the center and adding metal as needed. In this way large gaps or holes can be closed up.

A hole can be drilled in concrete by heating with the welding torch. No great depth can be drilled because of the torch tip heating. The concrete pops, explodes and flakes off. The success of an attempt to drill concrete depends upon the moisture content of the concrete. Do not use a concrete floor to weld on, for if the heat from the torch strikes the floor, it will flake off.

When some part is broken and the piece is missing, it may be necessary to cut out and weld on a piece from some old casting. Often the missing part can be built up with filler rod of about the right size and shape to take its place.

To cool a bearing, deep grooves were cut in the lining, copper tubing was placed in the grooves and the torch used to run in babbitt metal over the tubes and to fill the grooves. The bearing was then scraped in and the tubing connected to the water supply. The result was a cool bearing that previously had given endless trouble.

A pit developed in the slip ring on a generator. The cable lead was clamped to the generator shaft and for three days the shaft was used as a collector ring. When the unit could be shut down for several days, the pit was filled by welding and turned down in place. This repair saved the cost of a new slip ring, and as it was not removed for making the repair, the time saved paid for the materials used.

Saving an old exhaust head was easy in the following case: Installing a uniflow engine and a turbine made it necessary to increase the size of the exhaust line. Of course this called for a new exhaust head unless the old one could be made larger. This was done by cutting it lengthwise, spreading it apart and welding in a strip of sheet iron, thereby making the old head two sizes larger.

Fifteen short pieces of ¼-inch guy cable had been thrown away as too short to use or splice. By using a small tip these pieces were all welded together, making 40 feet of perfectly good cable.

A hot-water tank in the basement of one of the buildings had rusted through at the point where the steam coil entered the end of the tank. The thin section was cut out and a piece welded in. This saved the cost of two new tanks and their installation, for the building must have been built around the tank, as there was no room to get out or in with one of the same size and it would have been necessary to replace it with two of half size.

Repairing bags and blisters in boiler tubes saves much time and expense. In the case of a large bag in a tube, it is the writer's practcie to cut a hole through the center of the bag, heat and hammer the bag up to shape and weld up the hole. Small blisters are heated and hammered up. We have never had a repair made to a tube give any trouble or come down again. A ruptured tube can be repaired by welding, but if the rupture is long and difficult to get at, a new tube is the cheaper.

In cutting out tubes there is usually little space for the head of the cutting torch and it is difficult to see to cut the upper two or three inches of tube. The better way is to cut out a section, then cut from the inside out. This removes enough of the tube so that it can be driven and the ends pulled so as to miss each other. If there is any scale, it will have to be cleaned off in line with the cut, when cutting from the inside. If the baffles are left on the tubes, some support should be put in for the long ends, as the weight of the baffles may bring them down on the welder. The cost in time and material (gases) for eleven tubes was one-half hour and $1.70. The time saved was six hours cutting with a mechanical tube cutter, and twelve hours for two men removing and replacing brickwork not necessary when the torch was used.—"Power," July 6, 1926.

LIGHT ALLOYS OR STEEL?

While, on the one side, engineering developments of today call particularly for materials possessing greater strength at high temperatures, there are, in other directions, demands for materials combining great strength, at ordinary temperatures, with a minimum of weight. Such a demand

comes mainly from aircraft designers, but it is likely to be made also to an increasing extent in the future in many other branches of structural engineering, more particularly where heavy masses have to be started and stopped at frequent intervals. The reduction of weight in a structure can be achieved in two ways, either by the use of heavy materials having enhanced physical properties, or by employing materials of low specific weight and of fairly good physical properties. Typical of the first type are the high-tensile alloy steels, while the light alloys of aluminium and of magnesium represent the second. Between these two types there is a certain degree of rivalry, and such rivalry frequently leads to the most important advances.

The advantages which can be claimed for each of the two types are fairly clearly defined. On the side of the alloy steels we have the fact that steel of all kinds is a material very familiar to the engineer; while the light alloys are still more or less new to him. Furthermore, the production of steels is in the hands of old-established firms of high repute and the materials thus produced naturally profit from a sense of reliability which it is difficult for newer materials to acquire. The most vital factors, however, relate to the weight-strength ratio and to resistance to corrosion. In regard to the former, the "specific tenacity" of the strongest of the heat-treated alloy steels is decidedly higher than that of the best of the aluminium alloys as yet commercially available, while in resistance to corrosion the best of the rust-resisting steels or alloys of iron-nickel-chromium are undeniably superior to any light alloy. So far, the case would seem to tend overwhelmingly in favor of the alloy steels; and, in fact, a decision in their favor has actually been taken in regard to certain large airships by the Air Ministry. There is, however, a strong case for the defense of the light alloys. In the first place, there is the considerable difficulty of fully utilizing the high strength-weight ratio of the steels. Where large and light structures are concerned, weight can only be kept down by the use of exceedingly thin sections. But in the production of thin material of satisfactory quality relatively heavy costs are incurred, to which a corresponding cost in actual construction, owing to the careful handling and accurate jointing which are essential must be added. Finally, there is the difficulty in using and handling a structure built of very thin sections. Another point in favor of the weaker but much lighter material is that it allows of the use of bulkier sections and, from the point of view of rigidity and resistance to flexure in struts and columns, this is an important advantage. These, of course, are essentially questions of design and the difficulty of utilizing the high-tensile heavy materials may be readily overcome in some cases and may yet prove insuperable in others. In regard to corrosion, also, the position is rather more favorable to the light alloys than would at first sight appear. The development of processes for the protection of aluminium alloys from corrosion has now been carried so far that practical success can be claimed to a very considerable extent, both by means of electro-plating with zinc or cadmium and by means of "anodic oxidation," which is now widely entering into practice among aircraft constructors. While the invulnerability of the rust-resisting ferrous alloys is not perhaps quite reached, perfectly practicable results can be achieved, provided that the light alloys themselves are suitable and have been rightly treated.

The balance between the two types of material thus rests to a considerable extent—apart from the serious item of cost—upon the strength-weight ratio which is attainable. While it may be that even in heat-treated alloy steels large advances are still possible, there seems to be good reason to think that any early and marked progress in that direction is at least unlikely. With the light alloys, however, the position appears to be different. So

far as strength-weight ratio is concerned, the original high-strength light alloy of the "duralumin" type has not as yet been seriously surpassed. Numerous efforts in that direction have been made, but so far none of the claims that have been advanced have materialized into commercial production. But it seems improbable that the very first alloy of a new type to be developed should permanently remain the best that can be achieved. It is not too much to hope, therefore, that in the near future a light alloy considerably superior to duralumin may be produced, whether as the result of work done by the industry itself, or in one or other of the research laboratories which occupy themselves with these alloys. If that can be done, the balance of the case could easily be turned decisively in favor of the light alloy, so that it is a matter which should not be neglected. The whole aluminium industry, the research institutions and laboratories, and the principal users of light structural materials—aircraft constructors primarily—are concerned with the problem. Great progress in aluminium alloys for use as castings has recently been made, largely in this country; it is to be sincerely hoped that efforts to make equally valuable progress in wrought structural materials will not be wanting, and that they will be adequately supported by those concerned with the use of such materials. Safer and lighter aircraft alone is of such immense importance that we cannot afford to neglect any possibility of progress in that direction.—"The Engineer," May 21, 1926.

CORROSION, TARNISHING AND TINTING OF METALS.

If it has long been understood that oxidation is in many cases the primary factor of metallic corrosion, *i.e.*, the waste of metals due to chemical as distinct from mechanical causes, recent researches also indicated peculiar aspects of the protective action of oxygen, which has likewise been long known. The noble metals are said not to corrode because they are not easily oxidized. On the other hand aluminium is not corroded because it is easily oxidized, but covers itself with a fine film of oxide impermeable to oxygen which stops the further oxidation. A moist layer of iron oxide does not, however prevent the further destruction of iron. There are thus protective and non-protective oxide films and the recent work, notably of Mr. Ulick R. Evans, M. A., of Cambridge, who gave two lectures on the subject last month at the Royal Institution, prove that the corrosion preferentially attacks those portions to which the oxygen has no direct access, and not those exposed to oxygen.

Whether or not an oxide film is protecting depends upon its physical nature. Mr. Evans pointed out that when magnesium was heated, the layer of oxide formed occupied smaller volume than the metal, and oxygen gained further access through the pores in the oxide. In most other cases the oxide occupied the larger volume, and the rate of oxidation tended to slow down. But the film would be in a state of compression and liable to crack and to peel with alternating heating and cooling so that the oxidation of the metal could continue. When the oxide film did not peel, as on nickel-chrome wire, the film apparently added to the strength.

The first visible effects of heating a metal was sometimes a tinting or tarnishing of the surface. As the film thickness on iron increased, the blue light was first cut out by the interference of the light rays reflected from the outer and inner surfaces of the film, and the reflected light therefore appeared in the complementary color, yellow. On further oxidation the green, yellow and red rays were extinguished by interference, and the successive temper tints (of the reflected light) of steel were straw yellow,

brown yellow, rosy-mauve, dark blue, light blue, steely gray. Second and third orders of interference colors were then developed. Different steels gave different colors. The colors on other metals, which generally agreed with interference rules, might be disturbed by the superposition of different order films.

If these oxidation tints and the many similar effects such as the spreading of iodine vapors over silver, the tarnishing in moist or sulphatic atmosphere, etc., were really due to interference, their sequence should be reversed when the films were gradually thinned down instead of thickened. Because that could not be done mechanically by polishing, Mallock and others had questioned the interference explanation. But Mr. Evans had succeeded in effecting this thinning by electrolysis, and he showed, moreover, that the brilliant film tints of lead oxide floating on molten lead were reversed into the complementary colors when viewed in transmitted light. Further, Mr. Evans had produced the temper heat tints at ordinary temperature by combined oxidation and cathodic reduction on copper which, according to Tammann, always became covered by an invisible oxide film impervious to oxygen. The protective action of the oxygen film, the immediate effect of which was to render the metal passive, was demonstrated by placing drops of chemicals, copper or silver nitrate, ammonia, etc., on clean or tarnished metal; the specially cleaned metal or the metal thickly coated with cracked or pervious layer, corroded more rapidly than the metal carrying the invisible oxygen film.

The progressive corrosion is largely electrochemical, and Mr. Evans demonstrated in the second lecture, by very simple experiments, that electric currents are set up by local differences in the oxygen concentration at the surface, and that corrosion occurs at the spots of least (not greatest) oxygen concentration. He placed strips of the same metal in both the compartments of a simple diaphragm cell filled with water. Quite a strong galvanometer deflection was seen, as soon as air was bubbled through the one compartment, and the anode of the current was the strip to which oxygen had no access, or relatively less access. These currents due to differential aeration were stronger in solutions of salts, alkalis and acids, of course, than in water, but were always distinct. When a drop of salt solution was placed on iron, the metal was anodically dissolved in the center of the drop to which the air could not penetrate; the cathodic area outside the drop, exposed to the air, did not change, and where the anodic ferrous chloride and the cathodic alkali liberated met on the surface of the drop, they formed a membrane of ferrous hydrate which became oxidized. In dry air metals kept well, especially if polished; they corroded and tarnished in moist air, particularly if splashed or polluted with chemical vapors. As in the splashing, the irregular condensation of moisture on irregular surfaces promoted the differential aeration and local corrosion. Whether the corrosion spread depended also on the solubility of the resulting product. Most metals were porous, sheet metal often unequally on the two surfaces. Rolling and cold work, also surface polishing, closed the pores, but only partly; the corrosion afterwards followed the cavities parallel to the direction of rolling or the slag lines. Iron would swell on prolonged exposure to vapors of sulphuric acid, and could then be picked to pieces.

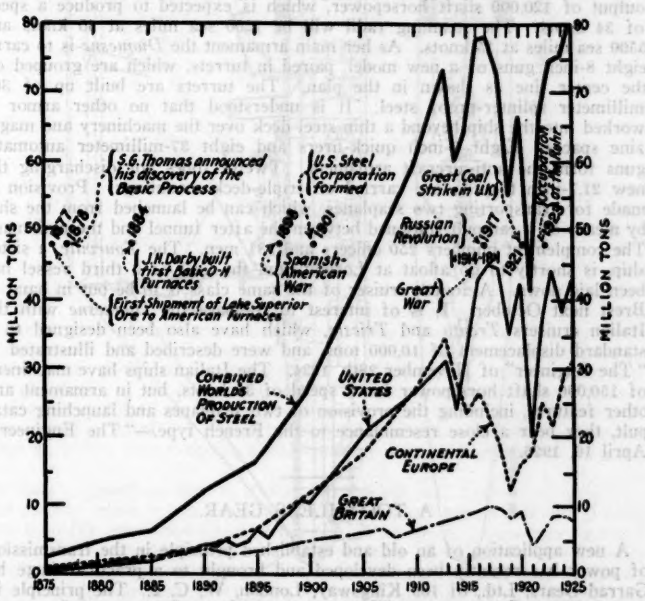
When sheet zinc was partly immersed in salt solution, the chief attack took place at some distance below the waterline, and the mixing of the zinc chloride and caustic alkali led to the formation of glove-like membranes of zinc hydroxide. Similar effects were observed with other metals immersed in solutions of salts or acids. The main attack might be in the splash zone intermittently covered by the liquid a little above the water-line, or at the true water-line, or below it. Mr. Evans, investigating this complex water-

line corrosion of iron, observed pronounced differential aeration effects. Just below the water line of the salt solution the iron became covered with a protective oxide film, which was cathodic to the unaerated anode surface lower down; the latter was therefore attacked. The water line itself was exposed to rapid corrosion because there the adhesion of the film was most liable to fail; the caustic soda creeping up protected the metal above the waterline. A sheet immersed in potassium chromate remained uncorroded, as it did also in sodium carbonate. But if the chromate or carbonate contained some chloride, the corrosion was very bad, and usually concentrated along the water line, and also on other spots.

These observations, which were in agreement with the results of experiments on the effects of drops of these chemicals, suggested the need of caution in the use of so-called rust inhibitors, such as sodium carbonate or potassium chromate. These salts should not be applied without due tests, and the loss of weight, which was frequently relied upon in corrosion tests, was a very misleading criterion. The total loss of weight might be small, in spite of bad local pitting and actual perforations.—"Engineering," June 4, 1926.

GROWTH OF WORLD PRODUCTION OF STEEL.

Fifty years of upward growth in the production of steel throughout the world is shown in the diagram. This growth, interfered with by the World War, showed in 1925 a new high record, at about 81,000,000 tons. The dia-



HALF A CENTURY OF STEEL OUTPUT OF THE WORLD AND OF GREAT BRITAIN, THE CONTINENT OF EUROPE AND THE UNITED STATES.

gram was shown by Sir Peter Rylands in connection with his presidential address before the Iron and Steel Institute of Great Britain.

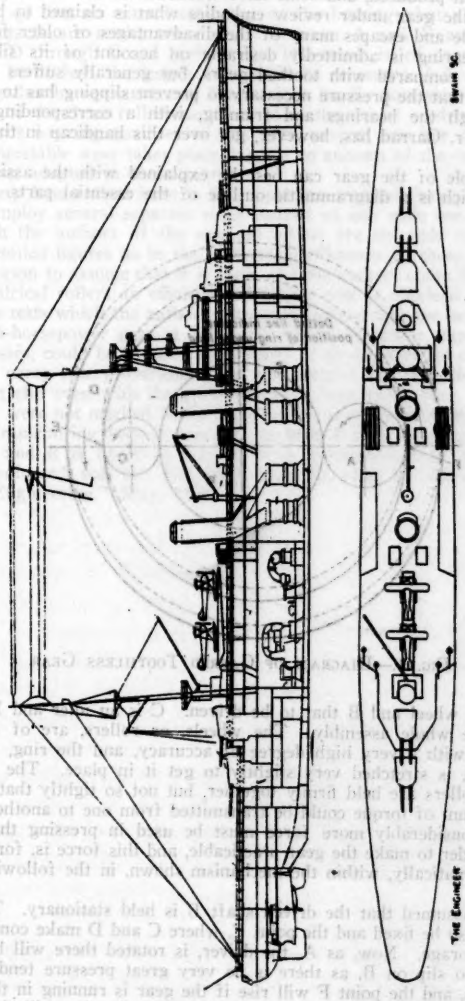
Particularly noteworthy are the close equality in production of the United States, Great Britain and the Continent of Europe, through the latter part of the nineteenth century, and the continuing close relationship between the United States and the European Continent in the first dozen years of the present century, during which Great Britain fell away from the group.—"The Iron Age," July 8, 1926.

THE FRENCH CRUISER *DUQUESNE*.

The diagram of the French warship *Duquesne*, which we are able to publish, is of special interest, as illustrating both the internal and superficial characteristics of a typical cruiser built in conformity with the Washington Treaty restrictions. The *Duquesne*, it will be recalled, was laid down at Brest during 1924, and went afloat on December 17th, 1925. She has a standard displacement of 10,000 tons, exclusive of fuel and reserve feed-water, but as her maximum fuel supply is 1200 tons, it is obvious that she will displace at least 11,500 tons when fully loaded. The leading dimensions are:—Length between perpendiculars, 607 feet; over all, 626 feet; breadth, 63 feet; draught, 20 feet. Her propelling machinery consists of four sets of turbines with reduction gearing and eight small-tube boilers, designed for an output of 120,000 shaft horsepower, which is expected to produce a speed of 34 knots. The steaming radii will be 1200 sea miles at 30 knots and 5500 sea miles at 15 knots. As her main armament the *Duquesne* is to carry eight 8-inch guns of a new model, paired in turrets, which are grouped on the center line as shown in the plan. The turrets are built up of 30-millimeter splinter-proof steel. It is understood that no other armor is worked into the ship beyond a thin steel deck over the machinery and magazine spaces. Eight 3-inch quick-firers and eight 37-millimeter automatic guns form the anti-aircraft armament. Twelve tubes for discharging the new 21.7-inch torpedo are carried on triple-deck mountings. Provision is made for transporting two seaplanes, which can be launched from the ship by means of a catapult stationed between the after funnel and the mainmast. The complement numbers 250 officers and 581 men. The *Tourville*, a sister ship, is shortly to go afloat at Lorient, and the keel of a third vessel has been laid down. A fourth cruiser of the same class is to be put in hand at Brest next October. It is of interest to compare the *Duquesne* with the Italian cruisers *Trento* and *Trieste*, which have also been designed to a standard displacement of 10,000 tons, and were described and illustrated in "The Engineer" of November 28th, 1924. The Italian ships have machinery of 150,000 shaft horsepower for a speed of 36 knots, but in armament and other features, including the provision of two seaplanes and launching catapult, they bear a close resemblance to the French type.—"The Engineer," April 16, 1926.

A TOOTHLESS GEAR.

A new application of an old and established principle in the transmission of power has recently been developed and brought to a practical stage by Garrad Gears, Ltd., of 109 Kingsway, London, W. C. 2. The principle in question is the use of the adhesion of two wheels in rolling contact for the transmission of power from one wheel to the other, with the object of pro-



THE FRENCH CRUISER "DUQUESNE."

ducing a speed ratio between two shafts. Many gears of such a nature have of course been produced, and some have worked with a moderate degree of success, but the gear under review embodies what is claimed to be quite a novel principle and escapes many of the disadvantages of older forms.

Friction gearing is admittedly desirable on account of its silence and cheapness, as compared with toothed gears, but generally suffers from the disadvantage that the pressure necessary to prevent slipping has to be transmitted through the bearings and framing, with a corresponding loss of efficiency. Mr. Garrad has, however, got over this handicap in the following manner.

The principle of the gear can best be explained with the assistance of Figure 1, which is a diagrammatic outline of the essential parts. In it A

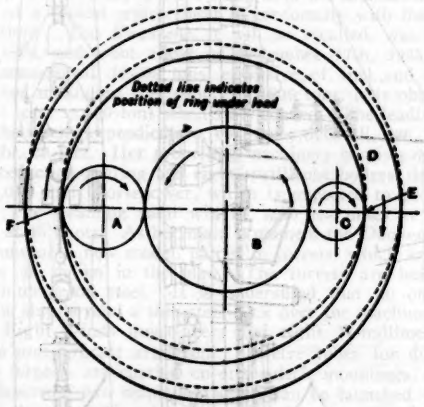


FIG. 1.—DIAGRAM OF GARRED TOOTHLESS GEAR

is the driving wheel and B that to be driven. C is an idler and D a ring embracing the whole assembly. The wheels, or rollers, are of hardened steel, ground with a very high degree of accuracy, and the ring, which is also hardened, is stretched very slightly to get it in place. The result is that all the rollers are held firmly together, but not so tightly that any appreciable amount of torque could be transmitted from one to another in this condition. Considerably more force must be used in pressing the rollers together in order to make the gear practicable, and this force is, fortunately, provided automatically, within the mechanism shown, in the following manner.

It may be assumed that the driven shaft B is held stationary. Then the idler C will also be fixed and the point E, where C and D make contact, will form an anchorage. Now, as A, the driver, is rotated there will be a tendency for it to slip on B, as there is no very great pressure tendency between the two, and the point F will rise if the gear is running in the direction indicated by the arrow. The result will be that the ring will rise, rotating about E, and the rollers will be gripped across a narrower part of the ring than the diameter. The grip is, in fact, limited only by the coefficient

of friction and the strength of the ring. It will be noticed that not only is the gripping force between the rollers wholly counter-balanced by the tension in the ring, so that no extra load is put on the bearings, but also that two driving contacts are made, one directly between A and B and the other from A to D through C and back to B. The result is, of course, that the rollers can be of half the length that would be necessary if A and B were pressed together by some external force. It is hardly necessary to point out that either of the three rollers can be used as the driver or driven member.

As regards wearing qualities of this gear, several sets are said to show that no appreciable wear takes place, largely on account of the comparatively light loading adopted in these gears.

When very long rollers have to be adopted to transmit large powers, it is usual to employ several separate rings instead of one wide one.

Although the authors of the original article are not able to give independent detailed figures as to the mechanical efficiency of these gears, there is every reason to assume that it is high, and the makers claim that with the plain cylindrical rollers an efficiency of 98 per cent is obtained at all loads, while some tests which the authors witnessed showed that the heat generated inside a 25-horsepower gear at full load, which, of course, represented the internal losses, could be supplied electrically at an expenditure of 200 watts. They will carry heavy overloads, and the authors say that they failed to make a set slip even with the assistance of a long pinch bar. Incidentally the rollers were not marked in the least by this severe treatment. A turbine gear for transmitting 1000 horsepower at 6000 R.P.M. is now being made in Midlands, and it is hoped to subject it to exhaustive tests very shortly. ("The Engineer," vol. 141, No. 3663, Mar. 12, 1926, pp. 304-305.)—"Mechanical Engineering," May, 1926.

BOOK REVIEWS.

MOTORSHIPS. By A. C. Hardy. D. Van Nostrand and Co., 8 Warren Street, New York City—Price $5.50.

This book should be of considerable interest to the engineer who has been "in steam" all of his career, but who now seeks information on oil engines and motorships. As it was written in England, more attention is paid to Continental practice than to American. The author's main object, as he says in his preface, is the motorship itself, its characteristics and problems. The subject is handled under four heads:—Construction, Arrangement, Evolution, and Operation.

Under "Construction", we find, after a short discussion of the theory of engine cycles, a description of various types of internal combustion engines as used to-day, not, however, in great detail. One would expect under this heading some description of the variation in hull structure due to the installation of oil engines.

The division "Arrangement" contains information on the auxiliaries necessary on a vessel propelled by oil engines, and the layout of machinery for various classes of vessels, examples being given of typical machinery spaces on cargo and passenger vessels.

The heading "Evolution" covers the question of connecting the prime mover to the propeller in order that the highest efficiency may be obtained from both. High speed of the prime mover in general, makes for lightness and efficiency. On the other hand, the slow running propeller is generally the most efficient. The three methods of transmission, mechanical (gears), hydraulic, and electric are discussed.

A chapter is devoted to the Scott-Still engine. This engine combines a two-cycle oil engine and a steam reciprocating engine, the top side of the piston operating as an oil engine and the bottom as a steam engine. The engine thus becomes double acting. The

Dolius, a cargo liner of 11,650 tons' displacement, has been equipped with such engines and has proved very reliable and economical.

The heading of "Operation" is really a description of various types of motorships, attention being devoted to passenger ships, cargo ships, ferry boats, and miscellaneous types.

A chapter on fuels and a glossary of motorship terms completes the volume.

A STUDY OF THE OCEANS. By James Johnstone. Longmans, Green and Co., 55 Fifth Ave., New York City—Price $3.75.

The title of this book is not particularly definitive of its contents. A study of the oceans means one thing to a seaman and navigator, another to the president of a shipping company and still another to a student. However, all three should find interest and profit in Dr. Johnstone's book, although it is patently written for the student.

A study of the oceans begins, of course, with their origin and this brings in the origin of the earth and its division into land and water.

The ocean of the ancients is touched on very entertainingly. It is difficult for us in the light of common school-boy information to visualize the groping in the dark of the ancient philosophers in their speculations on the earth.

The crossing of the ocean and all that it has meant to the people of the world from pre-historic days down to the present forms a most fascinating chapter. The absence of any material on ocean currents is noted. In view of their undoubted relation to the crossing of the ocean and the spread of various peoples, this is an important omission.

In the chapters on the circumpolar regions, the Atlantic, the Pacific and the Indian oceans, oceanography, history, geology and geography are presented in a way that makes one feel the urge to travel and see, as well as to procure the books suggested for future reading.

DIESEL ENGINES. By D. L. Jones. Norman W. Henley Publishing Co., 2 West 45th St. New York City.—Price $5.00.

"Diesel Engines" covers the subject very thoroughly from the view point of the practical operating engineer. It is illustrated with 241 photographs and sketches. The latter are especially clear and instructive.

As the book is written to meet the needs of the practical man, the discussion of theory is very brief. It is, however, very clear and easily understood. Unlike most books on Diesel engines, considerable space is devoted to the submarine engine, which makes its use, as a reference volume, of greater value to the Naval Service.

The entire field of Diesel engines is covered from the American point of view and comparatively little of European practice and development is included. The book covers progressively the elementary theory and principles of Diesel engines in general and then the construction and detail description of various typical well-known engines of American manufacture.

A chapter is devoted to indicator cards and testing and gives examples of engine irregularities as shown by typical cards. Other chapters include rules for operation, practical pointers, Lloyd's rules for vessels propelled by Diesel oil engines, and the various applications of Diesel engines to the Marine, Railroad and stationary field.

"STEEL SHIPS". By Thomas Walton, Revised by John King. Seventh Edition, 1926. xxiii + 70 + 270 Pages, With 53 Plans on Unnumbered Insertions. Lippincott-Griffin—Price $14.00.

This old favorite, which first appeared in 1901, has now attained the ripe dignity of seventh edition, which is somewhat of a record for a technical book. Many an old timer has been furbished up and reprinted after the lapse of a greater term of

years than twenty-five, but few have enjoyed a sufficient demand to justify the extensive revision which this book now receives. The original intention of treating the subject from a practical point of view has been well adhered to; although there is scarcely a sentence that has not been altered, the spirit of the book remains just what it was; indeed in many passages it may be doubted whether the paraphrase is an improvement over the original text. In any event it is no-where worse; the injection of a new personality into the authorship has resulted in much better arrangement and the rewording, which was probably felt to be necessary, is in harmony with other rearrangements which produce a more finished effect than former editions could show.

It is a new edition and not a new book. And in some respects revision might have gone further. Thus in Chapter II the references to iron plates and shapes have very properly been deleted, but nowhere in the two chapters on iron and steel do I find any reference to the progress that has been made in improvement of steel castings. One would hardly expect to find aluminum in the index, but surely brass is orthodox enough to have a place.

All the old diagrams will be found, though many are in new places. They make vivid contribution, for example, to the exposition of hull strength requirements. I wonder why the standard wave has a height of only 1/25 its length instead of the usual 1/20. We hear that it is still the practice to fair up a weight curve and to find a bending moment in still water, but the drawing of the "equivalent girder" has become unnecessary. By a typographical slip on p. 49 "Local Stresses" are made to appear as a subparagraph under the heading "Erections on Deck". It is just a bit of an anti-climax to wind up the talk about bending moment curves with $\frac{L.D.}{35}$. Although Shear in the Ship Girder comes in for a paragraph, the midship section is considered to have the same strength in hogging as in sagging. Deduction of area of rivet holes is a "needless refinement".

The plans are numerous, and cover the merchant field in a fairly complete way. Some of the general arrangements are

bulky and might have been spared. The *Campania* has been dropped in favor of the *Aquitania*. Trunk ships, cantilever ships, and other patented systems have been cut down to make room for such a novelty as the corrugated monitor. The chapter on longitudinal framing has been extended, but on examination it is found to be partly due to the inclusion there of the "Great Eastern". That the book is not quite up to the minute is shown by the failure of the bracketless tanker to find a place.

The wholly new chapter on "Modern Developments, Standard Ships, etc." looks interesting in the table of contents, but turns out to consist of 8 pages, which is hardly enough to do justice even to the two subjects of ship production in quantity and welding.

Like its big brother, by Campbell Holms, this book approaches the engineering problems of shipbuilding from the point of view of the classification societies rather than from the rational and mathematical side. In combination with Lloyds Rules, the book would offer a fund of information and data for study, and an aspiring young draftsman would find in it material enough for a year's study outside of working hours.

W. P. Roop.

ASSOCIATION NOTES.

At a meeting of the Council of the Society on May 20, 1926, the resignation of Captain W. T. Cluverius, U. S. Navy as a member of the Council was accepted, due to Captain Cluverius' assignment to sea duty. Captain E. L. Bennett, U. S. Navy was elected a member of the Council to fill out the unexpired term of Captain Cluverius.

The following members have joined the Society since the last issue of the JOURNAL:

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Chadwick, James H., Lieutenant, U. S. Navy.
Dana, Marshall M., Ensign, U. S. Navy.
Helber, Carlyle L., Lieutenant (j.g.), (C.C.) U. S. Navy.
Kaplan, Leonard, Lieutenant (j.g.), (C.C.) U. S. Navy.
McCall, R. F., Lieutenant, U. S. Navy.

ASSOCIATE MEMBERS.

Brierly, R. C., Fuel Oil Testing Plant, Navy Yard, Philadelphia.
Knapp, Harry F., 603 Wilkins Building, Washington, D. C.
Landberg, Erik G., 1443 Hamilton Ave., Trenton, New Jersey.
Stout, Arthur W., Todd Engineering Dry Dock and Repair Co., New Orleans, La.

Reprints of the article "Technical Aspects of the Loss of the U. S. S. *Shenandoah*" may be obtained from the Secretary of the Society at $1.00 each.

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